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THE NEVER FINISHED TASKS OF FEDERAL TAX REVISION

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(*Read April 21, 1949*)

I

THE Symposium on Taxation and the Social Structure, held by this Society four years ago, is an admirable collection of papers on some of the philosophical questions in the tax field; plus discussions of more earthy statistical and economic problems. How, then, can there be any unfinished business? Moreover, surely a lawyer can add little to the field save obfuscation; the Supreme Court has taught us that. Nevertheless, insistent demands for tax revision and reform assail us year after year, and few of us would be so hardy as to claim perfection for the federal revenue structure as it stands. What needs to be done and who can be drafted to do it? These are more mundane questions than my predecessors have discussed here, such as The Capitalist and the Social Structure, or Conflict and Harmony in Taxation; but they are no less worthy of consideration.

The primary purpose of the original federal income tax law was to raise money. It appears as an initial proposition that a law with such a purpose can and should be drafted by a lawyer. Cordell Hull, who claims the authorship of the first federal income tax law of 1913, had been a lawyer and a judge. His act, a little over fifteen pages long, was not, I think, clearer or better devised than current tax legislation, but it was certainly much shorter.

Mr. Hull's brief income tax law shows that even he was confronted with questions that a lawyer has not been trained in law school to answer. Just what is the income which is to be taxed? Is it simply cash receipts, or does it include benefits received as well? Does it include gifts? To answer such questions, should an accountant's technical skill be called into use, or an economist's, or both? The Supreme Court told us years ago [1] that

> in determining the definition of the word "income" . . . this court has consistently refused to enter into the refinements of lexicographers or economists and has approved . . . what it believed to be the commonly understood meaning of the term [2] which must have been in the minds of the people when they adopted the Sixteenth Amendment to the Constitution.

That is perhaps another way of saying that income is whatever the Supreme Court for the time being thinks it is.

Mr. Hull may have shared the view that income is what the Supreme Court or the common man thinks it is; but since his act contains inclusions, exclusions, and deductions from income, as well as exemptions, he must have had some notion in his own mind of what he regarded as income. For the most part, his major premises were unspoken, however, for the inclusions expressed in his statute are "gains, profits, and income derived from" salaries, interest, rents, dividends, etc.,"or gains or profits and income derived from any source whatever." Thus he left the main question—what *is* income—unanswered. The Supreme Court and the other federal courts have been trying ever since to answer that question—to fill in the blanks in the statute itself.

Whatever may have been true in 1913, the composition of a good federal revenue act in these times, when the income tax act alone will be employed to raise sums of money many times as much as the whole federal budget of the twenties or thirties, requires many more skills than a lawyer alone possesses. An accountant, an economist, a lawyer, a statistician, all are needed to answer the questions a thoughtful Congressman will ask while a revenue bill is being considered. Moreover, several different specialties in any one of these professions are involved. Legislative draftsmen, tax practitioners, administrative officials may

[1] *Merchants' Loan & Trust Co. v. Smietanka*, 255 U. S. 509 (1921).

[2] "'Income may be defined as the gain derived from capital, from labor, or from both combined,' provided it be understood to include profit gained through sale or conversion of capital assets." *Eisner v. Macomber*, 252 U. S. 189, 207 (1920). A little reflection will show that the quoted definition, though often reiterated, does not help at all in deciding the hard case; for example, whether a dividend paid in preferred stock or debentures or common stock is taxable income. Whatever its derivation, is it a gain? Does it make any difference what was the preceding capitalization of the company?

all be lawyers, and the experience of each one is often needed in the construction of a good revenue act. Thus, a particularly clever chief of staff will be needed to focus upon the task at hand the training and experience of many different kinds of men, no one of whom is competent to answer all the questions at issue. The staff work is further complicated, of course, by the immensely various family, commercial, and governmental situations to which provisions of the revenue act will apply.

There has not been sufficient recognition of the underlying complexity of revenue problems, nor of the prodigious increase in their complexity since 1913. It is curious, for example, that men still debate whether tax laws should be used as an instrument of social policy. The fact is that the tax laws are one of the chief elements that affect or control decisions on problems of corporate policy, as well as problems of family settlements. Moreover, that was the case before the advent of the New Deal. The impact of the tax laws on business policy or social policy lies not in the rates alone, but in the substantive provisions of the law. Let me illustrate with a few examples from the corporate field.

1. All the income tax laws from 1913 down to the present day have imposed surtaxes at progressive rates on individuals' incomes. The normal tax on corporate income in 1913 was the same as the normal tax on individual income, and there was no surtax. To offset the apparent incentive to accumulate corporate incomes, free from the individual surtaxes that would be imposed if they were distributed, and to prevent the loss of surtax revenue, the law contained a provision taxing undistributed corporate earnings to individual shareholders, if the corporation were formed or fraudulently availed of for the purpose of preventing the imposition of surtaxes on the shareholders. In subsequent income tax laws, a provision designed for the same purpose imposes a penalty on the corporation which improperly accumulates its earnings.

That provision is one of the earliest examples of loophole-closing in the tax law. No doubt its draftsmen thought only of the necessity of such a provision to protect the revenues, and were not then concerned about long-run effects thirty-five or fifty years later. Rates were very low, and tax impacts were small.

Now no one would urge that closely-held corporations should be perfectly free so to accumulate earnings that surtaxes on their wealthy stockholders are avoided. On its face, therefore, the penalty section is justifiable; and experience indicates that it is completely effective so far as the personal holding corporation is concerned. The present-day difficulties arise in cases of small or medium sized business corporations, not in cases either of large corporations or of personal holding companies.

The problem begins with a Bureau of Internal Revenue rule of thumb that a corporation should distribute 70 per cent of its earnings, unless it can demonstrate sound corporate reasons for retention of earnings. A large publicly-owned corporation can almost always show pressing needs for funds for development and expansion. The few personal holding corporations now in existence distribute nearly all their earnings as a matter of course. The penalty is most apt to be asserted against business enterprises trying to grow; enterprises whose stock is closely held because they are relatively unknown, and public financing is costly or impossible. Small business is generally forced to produce and to accumulate the capital required for its improvement and expansion.

Here we have, then, a conflict between the normal desire of Congressmen and Treasury officials to penalize tax avoidance; and the necessities of building up capital for corporate business. How can such a conflict be resolved? Who shall decide what interest is paramount? The question started as a legal one, for a lawyer likes to make his statutes and his contracts evasion-proof. It must be evident, however, that the question is not a legal one today.

2. Another subsection of the income tax law of more general application is that granting "a reasonable allowance for the exhaustion, wear and tear (including a reasonable allowance for obsolescence) (1) of property used in the trade or business, or (2) of property held for the production of income." The philosophy of the subsection is clear enough. If machinery, for example, is being worn out in the course of producing articles of commerce, part of the gross proceeds of sales will represent, in theory, payment for that fraction of the machinery which has been worn out in making the goods. Therefore, to arrive at the net income which is the subject of the tax, a deduction will have to be allowed not only for money spent on repairs, but for the depreciation of the machine. Depreciation is usually calculated on the basis of an estimate of the physical life of the depreciable property. Obsolescence, for which the statute also provides, takes into account the period of economic

usefulness of the property, which may be less, of course, than the physical life.

Now it is clear that the administration of the depreciation allowance has important economic consequences. If the allowance for depreciation is inadequate or non-existent, as is the case in Great Britain, the strong sanction of the income tax law, backed by its high present-day rates, is opposed to the replacement of old machinery with new, and particularly of obsolescent, but serviceable machinery with up-to-date. Thus the country's manufacturing establishments will not profit by the systematic introduction of the latest labor-saving devices, with consequent reductions in price to the consumer. Similarly, a rigid and mechanical administration of the depreciation deduction, an insistence by tax authorities that the depreciation claimed shall be conclusively established in each case (as seems to be our practice here) will discourage the replacement of buildings and machinery; and may further operate to discourage new investment in enterprises with large capital investments in plant and machinery. Replacement is further discouraged, of course, in times like these by the fact that the new machine will cost considerably more than the depreciation reserve accumulated over the years to provide its original cost. Consequently, thoughtful accountants are today considering what new methods may be adopted by a corporation to provide adequate depreciation reserves; and one day, if the price level continues high, we shall have advocacy of a new and more liberal deduction for tax purposes.

Implicit in the wording of the depreciation deduction, and in its administration by the Bureau of Internal Revenue, then, are economic problems of great significance to the industrial development of the country. One of the best ways to encourage a large investment in plant and machinery per worker employed would be a liberal statutory deduction for depreciation and a liberal policy in administration. Incidentally, such a policy would eliminate many of the costly tax disputes that occur today. Conversely, an inadequate depreciation allowance or a niggardly administration of the deduction will discourage additional industrial investment and advancement.

3. A final and striking illustration of the economic impact of the income tax law is the double taxation of corporate dividends. Here the economic impact is due, not to specific sections of the income tax law, nor to administrative action, but to the absence of any provision to prevent corporate earnings from being taxed once when realized by the corporation, and again when received by an individual stockholder as a dividend. The impact is emphasized by the facts that no other item of income is subjected to two taxes; and, in particular, that interest on corporate obligations, being deductible by the corporation, is taxed only once. Thus, corporations are given a powerful stimulus to raise new capital by issuing bonds or debentures rather than stock—a result that the S.E.C. works to forestall in areas committed to its charge. The tax situation as to corporate dividends discourages investment in equity securities, at a time when general governmental policy seeks to encourage maximum production. New capital for investment is known to be short, yet the tax law encourages the purchase of tax-exempts, not of common stock. The double tax can be defended as a revenue producer, but that fact is an unsatisfactory reason for perpetuating a tax result both inequitable and undesirable on policy grounds.

The demonstration that the income tax law is full of sections that have a decided impact upon business policy is proof of what is obvious to any thoughtful taxpayer. If the demonstration has any value, it lies in showing that decisions, whether administrative or legislative, to construct or to enforce the statute so as to collect the most revenue, may have results that are more costly to the economy than the revenue gain. Each of the cited provisions has revenue significance; in the aggregate, more than one billion dollars in revenue may be involved. The other side of the coin is the need in a free economy for constantly increasing investment in plant and machinery. The tax law need not offer incentives to new equity investment; but it should not stand in the way. As currently administered, the three named provisions do hinder, to some degree, our economic development, though perhaps not yet to a serious extent. Nevertheless, any one provision could result in serious harm; and over the years, with continued high tax rates, the adverse effects will probably become evident.

To return to my place of beginning, our revenue laws have not been examined and analyzed in Washington to an adequate degree from the point of view either of the economic premises of particular provisions, or of over-all economic effects upon the economy of the country. The first principle has been to draft a law that will produce the money, that is free from loopholes. Congress has sought to build laws that will operate fairly as between taxpayers and to adjust rates and exemptions in accord with current ideas of capacity to

pay. But there is still much work to be done by academic economists or by a council of economic advisers upon the economic effects of the tax laws, outside of revenue production. Such a task can be well performed only by men of broad experience, keen perception, and judicial objectivity, for the evidence is not easy to discover, the amounts involved are large, and the interests of our citizens are diverse. Today, nearly all of the comments on the economic effects of the revenue laws come from advocates and partisans, not from independent, objective students. The science of taxation is so complicated and specialized that neither citizen nor Congressman knows much about its economics anyway, and current public statements are more likely to confuse and to mislead than to lead to intelligent conclusions. Yet surely on such major issues, Congress should act advisedly, not blindly. Today too many of the economic premises of particular sections, and their economic results, are undiscovered and unknown. Here is a task, then, that needs to be performed by a competent scholarly group, in order that the best interests of the State may be fostered and preserved.

II

When a faculty club house was to be built years ago at the University of Chicago, I am told that an internationally famous geologist was most active in preparing and advocating specific plans for the building and designs for its exterior ornamentation. He was a geologist, but in this case, at least, he yearned to be an architect. That phenomenon is not unusual in academic circles. After a specialist has spent a lifetime achieving distinction as an economist, a physicist, or a geologist, his interest in another field of art or learning often waxes, while his interest in his own field wanes.

My discussion so far indicates that I myself suffer from the occupational disease I have mentioned, for what I, a lawyer, have suggested is more extensive investigation and consideration of the economic impact of the federal tax laws than has so far been made. Are there no unfinished legal tasks to be done as well? Have the revenue laws been completely perfected, viewed as a job of draftsmanship and of legal mechanics?

Congress was wise in deciding about thirty years ago to establish the offices of Legislative Counsel of the House and of the Senate. The Legislative Counsel and his staff serve as expert statutory draftsmen, on call by Chairmen of Committees of the respective Houses. Appointments to the staffs of the two offices have been notably excellent and entirely non-political. Some of the incumbents have remained in the office for considerable periods of time. One of the Legislative Counsel, indeed, has served as such for about thirty years.

For many years the revenue laws have been the particular care of the Legislative Counsel for the House, the man with the longest service. The actual drafting of a revenue bill has come to be the responsibility of three men: one of the Legislative Counsel, the Chief of Staff of the Joint Congressional Committee on Internal Revenue Taxation, and the Tax Legislative Counsel of the Treasury. Congress properly reposes great confidence in these men, to report to the respective fiscal committees, for example, the major questions of policy which have been developed, and to ask committee instructions thereon. On the other hand, the preparation of the sections of the revenue bill itself, and indeed of the committee report explaining its provisions, is wholly left to the drafting group, and is not performed by Congressmen. Of course, the drafting group may have the benefit of drafts of proposed amendments and of explanations prepared by interested Congressmen or outsiders.

Although the provisions of the Internal Revenue Code have been rearranged in the interests of simplicity [3] and codified [4] from time to time, the general character of the income tax provisions seems to have been pretty well fixed and determined in 1924. One of the fundamental questions underlying any income tax statute is whether its provisions shall be stated in terms of broad policies, or in terms of specifications of detail applicable to various income-producing transactions, to the various deductions, and so on. If the former course is followed, then the specific rules will have to come from decisions of the courts or from regulations and rulings of the administrative body charged with enforcement. Thus the job of working out and prescribing the specific rules will have been yielded by Congress to the other two branches of the Government. Moreover, there will be considerable delays before anyone can know just what are the rules applicable to particular concrete cases.

The draftsmen and Congress in 1924 took the second course: they made important changes in the income tax law as it affected corporate reorganizations and the taxation of the income from trusts, and the new sections were drawn in highly specific terms. Moreover, the statute was accom-

[3] As in 1928.

[4] In 1939.

panied by a lengthy, indeed a classic, explanation and committee report, explaining just what the law was intended to do, with examples. That scheme of draftsmanship, of elaborately specific provisions, has been followed ever since.

The policy has various consequences, some satisfactory, some unhappy. In the fullness of time, the income tax statute, by its very length and complexity, has grown beyond the power of a layman to understand. On the other hand, the ordinary lay taxpayer is concerned with relatively few provisions of the law, and these for the most part he can comprehend. It is doubtful whether the basic sections could be stated much more simply than they are, but certainly the language, with its encrustations over the years, could be improved. The most highly complex sections are for the tax lawyer; and if he will read carefully and will weigh every word, he will find a clearly marked, though narrow, path through the underbrush.

The fact that the statute attempts to deal with so many kinds of income-producing transactions in specific terms would lead a court less vigilant to protect the revenue than the present Supreme Court to conclude that other transactions are not to be taxed in the same way, because they have not been accorded special treatment. The Supreme Court of Mr. Justice Holmes's day, in 1930, held, however, that an assignment of income from husband to wife was ineffective to shift income tax liability to the wife, though there was no section in the Revenue Act providing specifically that income is taxable to the one who earns it, irrespective of preceding assignments; and though the contract of assignment long antedated the income tax amendment and so was not a tax-avoidance device.[5] The Supreme Court of Mr. Justice Douglas's day, in 1940, held that the income from a five-year trust, created by a husband for his wife as income beneficiary, was taxable to the husband and not to the wife, although trust income distributed to a beneficiary is normally taxable to the recipient. Indeed, Congress six years earlier had failed to adopt recommendations of the Treasury to accord to short-term trusts specific treatment similar to that provided for revocable trusts and trusts for the grantor's benefit; that is, to provide specifically in such cases that the trust income is taxable to the settlor, not to the beneficiary who receives the income payments.[6] In both cases, the Court relied upon the very general section of the law defining gross income as requiring income to be taxed to the man who owns or controls it.

The Revenue Act of 1924 made a particularly bold and extensive contribution to the law in its provisions relating to corporate reorganizations. The basis and exchange sections are both specific and complicated. It might be thought that Congress had stated precisely and fully every condition and qualification that it meant to apply. Yet here again the Supreme Court has inserted additional requirements of its own. Since 1935, it has been necessary that a reorganization shall have a valid "business or corporate purpose" [7] as well as that it shall satisfy the specifically stated provisions of the statutory definition of a reorganization. Further, the shareholders of the old corporation must "continue to be definitely represented in substantial measure" in the new one,[8] and it is not clear whether this requirement is superseded by the specifications later introduced into the statutory definition of a reorganization.[9] In a more recent case from this Circuit,[10] the Court held that a recapitalization "which is merely a vehicle, however elaborate or elegant, for conveying earnings from accumulations to the stockholders is not a reorganization under Section 112." That recapitalization *in form* fell within the statutory definition, but the Court was not satisfied that it did so in substance or in principle.

It appears, therefore, that the involved detail of the statute has not prevented the Court from searching for an underlying philosophy, a set of fundamental principles back of the specific sections and more significant than they in governing transactions and in resolving disputes. The Court has frequently stated that the substance, not the form, of the transaction controls. In abstract statement, this proposition seems almost axiomatic; but what it means is that the Court's idea of what the parties should be deemed to have done, and of what the underlying purpose of the statute should be deemed to be, will prevail over the letter of the law and over the actual facts of the transaction. Lawyers may urge that the taxpayer is entitled to be guided and taxed by the letter of the law, strictly construed, but that has not been the working rule of the Supreme Court, either before or since the new dispensation. Yet a judicial controversy about a specific issue is not the ideal way to work out broad legal principles applicable to many cases;

[5] *Lucas v. Earl*, 281 U. S. 111 (1930).
[6] *Helvering v. Clifford*, 309 U. S. 331 (1940).
[7] *Gregory v. Helvering*, 293 U. S. 465 (1935).
[8] *Groman v. Com'r.*, 302 U. S. 82 (1937).
[9] See Magill, Taxable Income (1945), 160 *et seq.*
[10] *Bazley v. Com'r.*, 331 U. S. 737, 332 U. S. 752 (1947).

nor is it easy for a lawyer or a business man to decide upon a course of action, when the rules are not literally those stated in the book, but doctrines to be worked out by the Court ten years from now.

The advisers of the Congress, notably the staff of the Joint Committee on Internal Revenue Taxation and the Treasury representatives, have ready access to a great volume of data and to many specific cases. These experts can analyze and use a hundred cases as a basis for working out a statutory rule, while the Court has only a single case before it. Hence, there is much to be said on practical grounds, as well as on abstract constitutional principles, for having the details of internal revenue rules worked out by Congress and the Treasury rather than by the Court.

Any judicial descendant of John Marshall, however, will prefer to deal in broad general doctrines, rather than in statutory minutiae. Would it be possible to attack and perhaps to solve this problem by drafting the statute to contain, first of all, a statement of the guiding principles which Congress wished to establish, plus, in the second place, a series of detailed rules for handling particular types of transactions? The revenue act might then bear some resemblance to the continental codes. If the draftsmen show the same skill in formulating such statements of general principles that they have demonstrated in drafting specific sections, the clarity and the dependability of the revenue act should be materially improved. In all probability, the federal courts would welcome a good clear statement of the legislative policy respecting such matters as the nature of income (does it include all benefits the taxpayer receives?); the scope of deductions (to what extent should accounting rules be applied?); as well as the obvious cases of reorganizations (is the statutory definition complete, or are courts justified in subtracting from it or in adding to it?); and of trusts (is trust income to be taxed in all cases as the statute provides, or are there cases in which the ownership of trust income is not specifically covered by the statute?).

The American Law Institute, under the leadership of Judge Herbert F. Goodrich and Harrison Tweed, has secured funds to finance the preparation of sample provisions of this general character. It is one of those tremendous tasks in which failure is more probable than success. In any event, however, the Law Institute group may be able to point the way to a new and better way of drafting revenue statutes. It should be able to finish at least a sample of provisions, drawn by experts who are not under the heavy pressure of an immediate legislative program requiring a bill to be ready for introduction next Tuesday. There is hope that such an example may be contagious; and out of it may come a new income tax law, whose basic philosophy will be set forth clearly enough for the business man, the lawyer, and the Supreme Court all to understand.

Treasury Regulations interpreting the revenue acts are almost as important in the day-by-day administration of those laws as court decisions or as the bare wording of the acts themselves. The Treasury has shown much independence from Congressional domination in preparing its interpretations. There have been notable instances in which the Treasury construction of the revenue act was directly contrary to the Congressional purpose as expressed in the committee reports. Such a conflict is as exasperating to taxpayers as it is difficult to resolve. Since the Treasury as the tax-collecting agency is one of two interested parties involved in any tax controversy, it is hardly in a position to be wholly objective in preparing in advance its official interpretation of the meaning of the law. Hence, one member of the House Ways and Means Committee has recently proposed that a new agency should be set up to prepare the official regulations.

That remedy seems too drastic, however, for the proposed agency could not have the advantage of the Treasury's rich experience and voluminous files and other data. Would it be better to permit the Regulations to be drafted by the staff of the Commissioner of Internal Revenue, as at present, but provide for approval before publication, not by the Secretary of the Treasury (the present requirement), but by the Joint Congressional Committee on Internal Revenue Taxation? Such approval would involve some delay and possibly some controversy between the Bureau draftsmen and the Congressional experts. On the other hand, the Joint Committee staff is a competent and experienced group, responsive to the will of Congress. Their review and approval would go far to insure both that the regulations correctly interpret the Congressional will and that they are fairly drawn. The taxpaying public would be almost sure to gain, since it would obtain regulations more nearly consonant with the statute, as well as more objective interpretations of the law.

Each of the tasks portrayed in barest outline in this paper is monumental. To accomplish any one of them satisfactorily will require the best intelligence of well-trained men applied for a considerable period. But who can doubt that the tasks are

worth while? No corporation executive would hesitate to spend many thousand dollars to perfect a plant or a process that promised to turn out a better product at a lower price. What can we afford to spend to perfect a machine that produces forty billions of dollars each year, and has been producing many billions for many years; that has a corresponding impact upon nearly every activity and transaction in the land? The cost is not a serious deterrent, but the tasks will not even be begun without imagination to perceive that they exist and without driving intelligence to see them through. My purpose today is to stimulate the thinking that some day will crystallize into resolution to move forward on these jobs, whether or not defined precisely as I have set them out.

MORE LIGHT ON THE LONDON NAVAL TREATY OF 1930

CONYERS READ

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WITHIN the past three years both the U. S. government and the British government have published state papers throwing fresh light upon the negotiations which eventuated in the London Naval Treaty of 1930. Records of the formal conferences and of the formal committee reports connected with these negotiations were published both by the U. S. and by the British governments in 1930. They have been adequately surveyed by Professor Toynbee in his *Surveys of International Affairs* for 1929 and 1930. But behind this facade, most of the knotty problems involved were untangled in off-the-record discussions between individuals and between delegations. It is upon this back-stage business, both before and during the negotiations at London, that the newly published British and American state papers throw light. The purpose of this article is primarily to examine and appraise this new material.[1]

I. PRELIMINARY ANGLO-AMERICAN CONVERSATIONS

The question of the initiative in these negotiations calls for some revision. Mr. Hoover himself boldly announced his claim to that distinction in his press conference, 12 April, 1930,[2] and subse-

[1] The state papers from U. S. State Department files appeared in *Foreign Relations of the U. S.*, 1929, vols. i and iii; 1930, vol. i. The pertinent British material from Foreign Office files appeared in *Documents on British Foreign Policy*, 1919–1939; Second Series, vol. i, edited by Prof. E. L. Woodward and Rohan Butler, London, 1946. Hereinafter, the British documents will be cited as *Woodward*, the U. S. documents as *U. S.* with date and volume. For a discussion of these sources, *cf. Amer. Hist. Review* **54**: 307–314, 1949.

Printed materials on the subject which appeared during and immediately after the conference are to be found in *Annotated Bibliography on Disarmaments*, published by League of Nations (Geneva, 1931). These include the official record of the conference issued by the British government (London Naval Conference, *1930 Documents*, London, 1930) and the United States government (London Naval Conference 1930, *Proceedings*) together with two white papers, 1930 Cmd. 3485 and Cmd. 3547, and the *Hearings* before the U. S. Senate Committees on Foreign Affairs and on Naval Affairs, London Naval Treaty of 1930 (Washington, 1930), which give a good idea of the American point of view. These hearings were for the most part cross-examinations of prominent naval personnel, particularly those who were members of the general board of the U. S. Navy and those who went as naval advisers to the London conference. They contain a good deal of information about the so-called yardstick and the official position of the general board on the treaty—with a reprint of their memorandum of Sept. 11, 1929. (Committee on Naval Affairs, *Hearings*, 502 ff.) They reveal a considerable amount of difference of opinion among high ranking naval officers regarding the relative merits of the 6″ and the 8″ gun cruisers. No official publications on the conference appear to have issued from the Japanese or the French or the Italian governments. Of secondary accounts the most important perhaps are Arnold Toynbee's in the Royal Institute of International Affairs, *Survey of International Affairs*, 1929, 1930, and John Wheeler-Bennett, *Disarmament and Security since Locarno 1925–31* (London, 1932). Chaput's more recent book, *Disarmament in British Foreign Policy* (London, 1935) adds little or nothing, though it contains a useful bibliography. Walter Lippmann's article, "The London Naval Conference," which appeared just after the treaty was signed, in *Foreign Affairs*, July, 1930, is a clear, concise and in the main an accurate statement of the issues and the outcome. The chapter on the subject in *American Foreign Relations*, ed. by Council on Foreign Relations (1931), 319–415, is less helpful. Other pertinent articles in French, Italian, and English reviews are listed in the League of Nations *Bibliography*, noted above. Charles G. Dawes, *Journal as Ambassador to Great Britain* (N. Y., Macmillan, 1939), nearly half of which is devoted to the negotiations in London, contributes a good deal of local color, and prints a number of documents but adds little to what is known from other sources. What is definitely a desideratum is a careful review of public opinion towards the treaty, particularly in England, France, and the United States, as recorded in the newspaper press. William S. Myers, *The Foreign Policy of Herbert Hoover, 1929–33* (N. Y., Scribner, 1940) contains some material from Mr. Hoover's unpublished papers. There is an elaborate account of the conference in Drew Pearson and Constantine Brown, *The American Diplomatic Game* (N. Y., Doubleday, Doran, 1935), pp. 65–171. It is entirely undocumented and much of it is demonstrably at variance with the known facts, but it is very suggestive and does make clear that newspaper men, particularly E. P. Bell and P. S. Mowrer of the Chicago *Daily News* played a larger part in the whole business than the official documents reveal. Harold Nicolson, *Dwight Morrow* (N. Y., Harcourt, Brace, 1935) discusses the activities of that statesman at the conference. Henry L. Stimson, President Hoover's Secretary of State, who was the leader of the U. S. delegation at London, records his observations on the negotiations in Stimson, H. L. and McG. Bundy, *On Active Service in Peace & War*, 162–174 (N. Y., Harper, 1948).

[2] Myers, W. S., *Foreign Policy of President Hoover*, 91.

quently repeated that claim in *The Problems of a Lasting Peace.*

Actually the initial impulse seems to have come from the League of Nations at Geneva in connection with the deliberations of the Preparatory Commission on Disarmament. Hugh Wilson, U. S. Minister to Switzerland, reports an interview with Mr. Colban, chief of the Disarmament Section of the Secretariat of the League on 9 February, 1929 in connection with the preparation of agenda for the 6th Session of the Preparatory Commission. Colban expressed his desire to get some suggestions as to procedure from the U. S. which was represented on the Preparatory Commission. The response from Mr. Kellogg, in the last days of the Coolidge administration, was brief and non-committal.[3] But not long after Mr. Hoover's inauguration Mr. Kellogg wrote again and set forth the attitude of the U. S. government towards the various matters proposed in the agenda. Kellogg went at length into the matter of Naval Armaments, in fact foreshadowed the speech which Hugh Gibson subsequently delivered at Geneva, 22 April, 1929.[4] This speech is usually taken to be the formal reopening of the whole question, and there can be little doubt that it was prompted by President Hoover. It did not, however, go beyond the frame of reference of the Preparatory Commission, and was intended merely to guide Mr. Gibson in his discussions. But not many days after Kellogg had sent Gibson his instructions, the British embassy at Washington submitted a memorandum to the State Department [5] in which the British, evidently anticipating that the Preparatory Commission would get nowhere, proposed: (1) that each power involved submit its own programme for limitation of Naval Armaments; (2) that the U. S. and Britain discuss their points of difference between themselves.

The British evidently felt that by some device or other the discussion of naval disarmament at Geneva should be postponed until the British and the Americans had time to discuss their differences. With this policy the U. S. government expressed itself in accord. Clearly the U. S. government had accepted the idea as early as 4 May. Two days later, at the meeting of the Preparatory Commission at Geneva, the British and the Japanese delegates, supported by the U. S., suggested that consideration of naval problems be postponed until the interested powers expressed their readiness to discuss them. The result was that the Preparatory Commission adjourned, to meet again at the call of the chair.[6]

In assigning responsibility for the resumption of negotiations on the question of naval disarmament, it may therefore be said that the issue was raised in connection with the preparation of the agenda for the sixth session of the Preparatory Commission, that a definite attempt was made to interest the U. S. in the agenda, but that until Mr. Hoover had succeeded Mr. Coolidge as President of the U. S., the State Department took a very cool and detached attitude, though it was of course represented on the Commission. Mr. Hoover, as soon as he became President, interested himself in the matter and his Secretary of State (at that time Kellogg, later Stimson) outlined the position of the U. S. This outline became the basis of a speech before the Commission delivered by Mr. Hugh Gibson, the American representative, on 22 April. Evidently Mr. Hoover at first contemplated working through the Commission. The British, however, felt that nothing much would be gained by public discussion before the Commission until the U. S. and Britain, the principal naval powers, had attempted to settle their differences. It was the British idea that these preliminary discussions should be intimate and confidential but the State Department demurred and it was accordingly decided to keep the representatives of the other powers, Italy, France, and Japan, *en rapport.*[7] In short, Hoover supplied the impulse, the British the direction of effort.

When this position had been reached towards the end of May, it was decided to await the outcome of the British elections before proceeding further. Hoover, meanwhile, had appointed General Charles G. Dawes (of the Dawes plan) as U. S. Ambassador to Great Britain, 10 April, 1929 and it was expected in Britain that Dawes when he arrived would be commissioned to open the negotiations.[8] The British election resulted in the fall of the Conservative government and the return to power of the Labor party, which, how-

[3] *U. S., 1929,* i, 65, 69.

[4] *U. S. 1929,* i, 91.

[5] 28 March, *U. S. 1929,* i, 78. Actually Gibson appears to have discussed an Anglo-American agreement with Craigie and Cushenden at Geneva. *Cf. U. S, 1929,* i, 189, 202; *cf.* also *Woodward,* 14, in which Craigie refers to conversations at Geneva on the *yardstick.*

[6] *U. S. 1929,* i, 100, 104.

[7] *U. S. 1929,* i, 100–101, 110. Mr. Stimson, in recording his memories of the situation later (Stimson, H. L., and McG. Bundy, *On Active Service,* 164), emphasizes the leading part played by Mr. Hoover.

[8] *Ibid.* i, 110, n. 1.

ever, could not command a majority of the whole house, and depended for its continuance upon the support of the Liberals. Ramsay MacDonald became Prime Minister on 5 June. He was a pacifist by temper, and utterly devoted to the promotion of peace and disarmament. Certainly the change in party in England was a very auspicious beginning for the negotiations, especially since the Baldwin government which preceded it had not only failed to reach any agreement with the U. S. government on the subject at Geneva in 1927 but had aggravated American distrust of British intentions by the so-called Naval Conversations with the French in 1928.[9]

General Dawes reached London 14 June, 1929, and held his first conference with MacDonald two days later. Already at Geneva, following Gibson's address, the British had expressed their approval of: (1) reduction as well as limitation in naval construction; (2) application of the principle of limitation to all categories of ships; (3) application of a "yardstick" for measuring comparative strength of large and small cruisers; (4) acceptance of the Kellogg pact as a new directive to the work of disarmament.[10] This involved important concessions to the American position, and, particularly in (1) and (2), indicated that the British were prepared to yield upon the two points which had been largely responsible for the failure at Geneva in 1927 and the unsavory aspect of the Anglo-French compromise of 1928.

One point which the Americans insisted upon from the start was the principle of parity as between the U. S. and Britain. Stimson began harping on it early in May and in his dispatch to Dawes of 27 June declared it to be a *sine qua non*.[11] There is no sound reason for believing that Britain had any desire to withhold it, though Stimson's reiteration indicates some nervousness about the matter. In any case MacDonald gave complete assurance upon the point in his interview with Dawes, 27 June.[12]

The early conferences between Dawes and MacDonald throw interesting light upon the Prime Minister's approach to the problem. MacDonald evidently assumed that the U. S. and Britain were already in agreement on fundamentals and could go ahead without delay. He himself contemplated an early visit to America, and it was part of Dawes' business to point out to him that such a visit had better be deferred until a U. S.-British agreement had been worked out in greater detail.[13] MacDonald seems to have felt also that a five power conference could be called almost at once and suggested 22 July as the date. But the U. S. government took the position that the technical questions involved called for much greater deliberation. And Dawes brought MacDonald to agree.

Another point which emerged in these preliminary discussions was that the United States anticipated opposition from France and Italy, and wished to go ahead with a three power agreement if they proved unsympathetic. The British did not object though they felt strongly that no such contingency should be mentioned until the five power conference had demonstrably failed. Dawes and Gibson evidently believed that if the three major naval powers agreed Italy and France would fall in line. But the British were plainly reluctant to proceed without France, and they expressed the opinion that anything like a substantial reduction in naval strength would, from the British point of view, be much more difficult without the concurrence of the two continental powers.

In a letter to Dawes of 8 July MacDonald outlined the position reached to date:[14] (1) the Kellogg Pact, accepted as the starting point; (2) reduction as well as limitation of armaments the objective; (3) parity between U. S. and Britain assumed; (4) parity as between different categories of war ships to be adjusted by an agreed "yardstick";[15] (5) Japan, France and Italy to be

[9] In consequence, the U. S. Congress had undertaken a programme of naval building which emerged, 13 Feb., 1929, in a bill authorizing the construction of fifteen 10,000 ton cruisers. *American Foreign Relations, 1930*, 345–400.

[10] *U. S. 1929*, i, 96, 117.

[11] *U. S. 1929*, i, 101, 108, 137; *Woodward*, 17.

[12] *U. S. 1930*, i, 139.

[13] *U. S. 1929*, i, 117. Dawes says that MacDonald brought up the matter of a visit to America at their first interview. Henderson in writing of the interview to Esmé Howard says that Dawes raised the question (*Woodward*, 9). Pearson and Brown maintain that Hoover sent E. P. Bell of the *Chicago Daily News* to London in March to influence the British Press on Anglo-American friendship and to sound out the possibility of Mr. Baldwin's (then Prime Minister) visit to the U. S. (*The American Diplomatic Game*, 72). After Baldwin's fall Bell, after some initial difficulties, made the same proposition to MacDonald (*ibid.*, 75). There is no confirming evidence on this, though according to A. Henderson, MacDonald declared in his first interview with Dawes (*Woodward*, 9) that the initiative in the matter did not come from him. Dawes reports an interview with Bell immediately after he arrived in London but throws no more light on the subject than to remark that Mr. Bell was close in the confidence of the President (*U. S. 1929*, i, 119).

[14] *Woodward*, 19; *U. S. 1929*, i, 140.

[15] MacDonald suggested that it would expedite matters if U. S. ideas on the subject were forwarded. Gibson had

kept informed of our progress "in ways which we can decide from time to time."

The reply of the State Department accepted 1, 2, 3, and 5, but hesitated over 4. Stimson thought the technical question of "the stick" called for more mature consideration. He suggested that in measuring combatant strength, displacement, guns and age should be the determining factors. He ruled out protection, speed, and habitability as of secondary importance.[16]

MacDonald professed to be delighted with this reply. He wrote to Dawes on the nineteenth of July that all that was needed to make the agreement complete was a yardstick.[17] But Mr. Secretary Stimson was of another mind. He pointed out that parity as between the U. S. and Britain on submarines and destroyers could only be achieved by large scale scrapping on the part of the U. S., and this could only be predicated on an equally drastic action by Britain regarding cruisers. As he estimated it, the total cruiser tonnage of Britain, in service, in construction and authorized amounted to 402,800 tons as against a total of about 300,000 tons of the U. S. The gap, as Stimson observed, was a wide one, hardly to be bridged by the mere application of a yardstick. It became therefore a question as to what the level of parity in cruisers should be—in a word, would the U. S. have to build up or would Britain scrap? This, as Stimson observed, was the issue on which the Geneva Conference had foundered. As Stimson saw it either the U. S. must proceed to the completion of its whole programme of cruiser building or Britain must not only check her present programme of construction but must build no more cruisers before 1936.[18]

MacDonald's first disposition was to dispute Stimson's figures. A little later (29 July) he submitted a private and personal memorandum to Dawes, in which he proposed for Britain fifteen 8″ gun cruisers, and forty-five 6″ gun cruisers, for the U. S. eighteen 8″ gun cruisers, and [in addition to what she already had] ten 6″ gun cruisers.[19]

These proposals, which reached Stimson on 29 July, came with something of the force of a shock. "They are," he wrote to Dawes on 31 July, "practically no real modification of those made at the Geneva Conference and it does not seem to me that they offer any hope of agreement." As Stimson figured it out, what MacDonald suggested was a total cruiser strength for Britain of 376,226 tons as against a total strength for the U. S. of 250,000 tons, plus ten 6″ gun cruisers which the U. S. Navy Department did not want. Stimson, in an aside to Dawes, raised the question as to whether MacDonald was springing the old Admiralty programme simply in order "to try us out." [20]

Dawes did not think so. He was disposed to defend MacDonald's position on the grounds that he could hardly afford to antagonize both the Opposition and the Admiralty. "Up to the present," Dawes wrote, "he has shown himself as a statesman who is ground between the millstones of his own Admiralty propositions and the American proposals."

Hugh Gibson, who was associated with Dawes in London at this juncture, was of substantially the same opinion. Gibson insisted that the situation was far from hopeless and proposed that further discussion be referred to a personal conference between MacDonald and Hoover. He pointed out that MacDonald was very anxious to reach some sort of an agreement before the British parliament reassembled in November after the summer recess and therefore urged that the two leaders meet together as soon as possible.[21]

Dawes did not agree with Gibson on this point and neither did Stimson. Both of them felt that major points at issue should be settled before MacDonald went to America.[22]

The first week in August marked the first serious crisis in the negotiations. It began to look as though MacDonald, whose political position was always precarious, would not take the risk of breaking with the Admiralty, and that the negotiations would bog down on the same issue that had caused the failure at Geneva. The issue of course was not parity—it was the level of parity. The British presented what they regarded as an irreducible minimum for their own imperial purposes, and were perfectly willing to allow the U. S. to build up to the same level. The situation was aggravated by the fact that the British cruiser fleet was a fleet in being, the U. S. cruiser fleet still

told Craigie at Geneva (according to Craigie) that the U. S. version of the yardstick had already been worked out and was in the President's safe in Washington (*Woodward*, 14).

[16] *U. S. 1929*, i, 141, 145.

[17] *U. S. 1929*, i, 149; *Woodward*, 24.

[18] *U. S. 1929*, i, 151, 155.

[19] *Woodward*, 28, 33; *U. S. 1929*, i, 165.

[20] *U. S. 1929*, i, 168, 170.

[21] *U. S. 1929*, i, 176.

[22] *U. S. 1929*, i, 179.

largely in the blueprint stage. Actually the British had reduced their terms. They talked about seventy cruisers at Geneva; in MacDonald's proposals of 29 July they had come down to sixty cruisers. But they were reluctant to allow the U. S. to outbuild them far in 8″ gun cruisers, not, they protested, because they feared war with the U. S., but because they expected Japan to demand ⅗ of U. S. 8″ gun cruiser tonnage and that might result in establishing parity between Japan and Britain in this class of cruisers.[23]

The impasse was broken by MacDonald himself who on 8 August proposed that parity should be established as of 31 December, 1936, at which date Britain would accept a cruiser strength of fifteen 8″ gun cruisers and thirty-four 6″ gun cruisers, this result to be attained by refraining from replacement as cruisers dropped out upon reaching the effective age limit of twenty years.[24]

The result was to clear the air almost immediately. "We feel," Stimson wrote to Dawes on 15 August "that the British and the U. S. are nearer real and complete agreement in the light of the true facts understood by both of us than we have ever been." Dawes recorded in his *Journal* 16 August, "The fact is that we are about agreed in principle." [25]

Stimson figured that MacDonald's revised figures accepted an aggregate cruiser tonnage for Britain of 330,000 tons and he suggested that parity could be achieved on that basis if the U. S. proceeded to build up to its full cruiser programme of twenty-three 8″ gun cruisers, which would bring the U. S. total cruiser tonnage to 300,000. The actual tonnage difference, Stimson suggests, could probably be reconciled by the application of a yardstick which would take into account other considerations than mere tonnage, such as age and gun power. He intimated that the U. S. might even accept twenty-one 8″ gun cruisers and make up the difference with 6″ gun cruisers.[26]

MacDonald in reply said he could never agree to twenty-three 8″ gun cruisers for the U. S. as against fifteen for Britain, and Stimson was disposed to argue the point. But actually both sides felt that the gap between them was no longer unbridgeable. MacDonald once more talked about his U. S. visit and the British Ambassador at Washington after consultation with Stimson on this subject wrote to Arthur Henderson, 21 August, "The Secretary of State said that he quite agreed that the visit would now be most beneficial and he telephoned later to say that [the] President was of the same opinion." [27]

Meanwhile Stimson and Hoover had been wrestling with the General Board of the Navy Department and had finally secured a commitment from them to the effect that on the basis of a British cruiser tonnage of 339,000 tons, the U. S. would accept parity with a cruiser fleet of twenty-one 8″ gun cruisers, ten 6″ gun cruisers (Omaha class) afloat, and five 6″ gun cruisers to be built, a total of 315,000 tons. Stimson expressed the opinion that the only point at issue was between twenty-one and eighteen 8″ gun cruisers for the U. S. and added that the President regarded this issue as small enough to be settled by compromise.[28]

There was still some hesitation on both sides, but after a conversation with Mr. Hoover, Vansittart wrote to MacDonald on 12 September that he believed the advantages of a visit more than counter-balanced the risk. On the same day, Henderson informed the British embassy at Washington that MacDonald would sail for America on the twenty-eighth. And sail he did.[29]

II. MACDONALD'S VISIT TO WASHINGTON [30]

MacDonald's visit to America was in the nature of a triumphant procession from the moment he set foot in New York until something like a week later when he left for Canada. The high point was the week-end spent with Mr. Hoover at his camp at Rapidan. Those present on the British side were, beside the Prime Minister, Sir Robert

[23] *Ibid.*, i, 171.

[24] *U. S. 1929*, i, 186; *Woodward*, 86. *Woodward* is the better text.

[25] Dawes, C. G. *Journal*, 55. On 15 August Esmé Howard spoke with Stimson about the proposed visit of MacDonald. Stimson said he saw no reason why MacDonald should not come (*U. S. 1929*, iii, 1–2).

[26] *U. S. 1929*, i, 190.

[27] *U. S. 1929*, i, 196, 203; *Woodward*, 46.

[28] 11 September. *Senate Committee, Naval Affairs, Hearings*, 502ff.

[29] *Woodward*, 77.

[30] The most interesting document on this subject is MacDonald's report to the Cabinet on his conversations with Hoover, 4–10 October, 1929, printed in *Woodward*, 100 ff. Unfortunately the concluding parts of the report are not printed. Documents appended to this report, printed in *Woodward* as Appendix A nos. 1–12 are illuminating. The documents on this subject from the State Department files in *U. S. 1929*, iii, 1–37 are much less informing, except as revealing Mr. Hoover's desires, none of which found expression, except in very ambiguous terms, in the press releases. For the most part the conferences took place at Rapidan, Virginia, Mr. Hoover's summer camp, 5–7 October. The only attempt at a secondary account of the conference is in Pearson and Brown, *op. cit.*, whose speculations are widely at variance with the story as revealed in the documents cited above.

Vansittart, acting as MacDonald's private secretary and R. L. Craigie, Chief of the American Division of the British Foreign Office. On the American side, beside Mr. Hoover himself, were Mr. Secretary Stimson and Joseph P. Cotton, American Under Secretary of State. There were no stenographers present and no naval experts. Conversations were quite informal and the only public statement which emerged from the conference was a joint press release from the White House (10 October, 1929) which is a masterpiece of amiable ambiguities.[31]

And yet the conference was not without its significance. For one thing it gave Mr. Hoover an opportunity to bring forward some ideas near to his heart and to raise some questions which were very near to the heart of Mr. Borah, Chairman of the Senate Committee on Foreign Relations, whose support of any treaty with Britain was a *sine qua non* to its ratification by the Senate. For the former, the idea of placing food ships in the same category as hospital ships in time of war was Hoover's particular darling. It was, MacDonald observed, "the direct outcome of his experiences and activities during the war." "The importance of this psychological factor," MacDonald added, "may be noted in passing." [32]

According to MacDonald, Hoover even hinted that if this matter of the immunity of food ships could be established by treaty it would be unnecessary to proceed further "into this complex problem of the rights of neutrals and belligerents." [33]

This latter question, which Americans spoke of as "freedom of the seas," a title less agreeable to the British, was so important in Borah's eyes that he inclined to the position that it should be tackled before any agreement on naval disarmament. Here was another psychological factor. MacDonald himself was definitely opposed to any statement on either issue, both of which he regarded as loaded with dynamite. His colleagues in the British Cabinet were adamant on the subject, and though both MacDonald and Vansittart took the position that if something were not said about these matters the treaty would probably be defeated in the Senate, "owing to an antiadministration campaign led by Borah," Hoover finally consented to omit any mention of them.[34]

Another issue which Hoover raised was that of British naval stations in close proximity to the U. S.[35] There had been a good deal of anti-British propaganda on this subject in the American press and Hoover thought a statement should be made regarding it, to the effect that the General Board of the U. S. Navy saw no menace in these stations as presently manned and fortified and the British government undertook not to alter them in such a way as to constitute a menace. As a corollary to such a declaration the U. S. agreed not to increase fortifications so as to menace Britain in the Eastern hemisphere—an empty gesture, as Hoover admitted, but perhaps of some face-saving value as a *quid pro quo* from the point of view of British public opinion. But this in turn was definitely rejected by the British Cabinet, and Hoover once again acquiesced in omitting it.[36]

The outcome was that in the Press release, mention of these troublesome matters was reduced to the following:

> We approach old historical problems from a new angle and in a new atmosphere. On the assumption that war between us is banished and that conflicts between our military and naval forces cannot take place, these problems have changed their meaning and character and their solution, in ways satisfactory to both countries, has become possible.
>
> We have agreed that these questions should become the subject of active consideration between us. They involve important technical matters requiring detailed study. One of the hopeful results of the visit which is now terminating officially has been that our two governments will begin conversations upon them following the same method as that which has been pursued during the summer in London.[37]

Nothing could be more non-committal. The outcome on all three troublesome questions was a complete victory for MacDonald. It must be remembered of course that the best account we have of the conference is from MacDonald himself, but there is nothing in the memoranda on the subject printed by the State Department inconsistent with this interpretation. Probably the deciding factor was the firm stand taken by the British

[31] *U. S. 1929*, iii, 35.

[32] Hoover's position is stated in a Memo dated 6 October (*U. S. 1929*, iii, 7) elaborated in another memo by Stimson dated 7 October (*ibid.*, 22).

[33] *Woodward*, 110. This view of the matter was supported by Sir R. Vansittart (*Woodward*, 118).

[34] *Woodward*, 112.

[35] There appears to be no grounds at all for the position taken by Pearson and Brown, *op. cit.*, 103, that the matter originated with MacDonald.

[36] *Woodward*, 113; *U. S. 1929*, iii, 19.

[37] *U. S. 1929*, iii, 34.

Chiefs of Staff which was transmitted by Henderson from London and was shown to Hoover.[38]

It is no wonder that the details of the story were carefully suppressed on both sides. It would certainly not have helped the cause of the treaty in the U. S. if they had been revealed.[39]

So far as the problem of naval disarmament was in question the most important achievement of the Rapidan meeting was a memorandum by Mr. Hoover dated 6 October. It must be taken in conjunction with a memorandum from Mr. Craigie of the British Foreign Office of the same date.[40] The gist of it appears to be that parity might be achieved with fifteen 8″ gun cruisers and thirty-five 6″ gun cruisers for Britain and eighteen 8″ gun cruisers and seventeen 6″ gun cruisers for the U. S.[41] Hoover seems to have felt that these figures offered a way out, though he was strongly of the opinion that "it was undesirable to submit these figures in such places as they would be likely to become public, or that would start speculation, and that we should hold them confidential until we arrived at the conference." MacDonald's observations on the subject, in his memorandum to the Cabinet, were that any hope of solution on some such lines as these would depend "on the personal influence of the President, and the degree of pressure which he will exercise in their direction must depend in turn upon his being kept in the frame of mind in which he was when we left Washington." [42]

All things considered this appears to have been another victory for MacDonald. But he made no such claims in his report to his Cabinet. He still spoke of gaps to be bridged, omitted any mention of concession of tonnage on his part, and concluded, "We both realized that, even if a full agreement on the cruiser question were possible at this stage, its announcement would further arouse the sensibilities of other powers." [43]

The interval between Ramsay MacDonald's visit to America and the assembling of the five power conference in London was filled chiefly with conversations between the two principals and the three other powers involved, chiefly with Japan and with France. Invitations to the conference had gone out 7 October [44] and all three powers had indicated their willingness to participate. Of the three, France showed the greatest reluctance. Italy was never a factor of major importance except as her position influenced France. Japan was cordially disposed towards the conference, but insisted upon an increase in her power ratio from the 5–5–3 of the Washington treaty on battleships to a 10–10–7 on cruisers.

The French attitude was determined by a number of factors. In the first place, she was distrustful of Anglo-American conversations in general and this distrust was increased by the evident desire both of the U. S. and of Britain to hang the naval conference on the Kellogg Pact and not on the League of Nations. If there was one basic factor in French foreign policy at this time it was a resolute support of the League. The British were able to demonstrate that the forthcoming conference was really the offspring of the Preparatory Commission of the League and enjoyed its blessing, but France was not satisfied. On top of that the French had been considerably chagrined by what they regarded as the high-handed methods of the U. S. at the Washington Treaty,[45] and were fearful that they might be faced at London with a *fait accompli* dictated by the Anglo-Saxon powers.[46] There was moreover a strong feeling in

[38] *Woodward*, 121.

[39] MacDonald's account of his mission to America in the House of Commons was a model of discretion. The only reference he made to the difficult issues raised at Rapidan was prompted mainly by a sort of gentleman's agreement with Hoover that he would "go into the whole subject on my return to London" (*Woodward*, 112). The actual passage in the speech runs as follows: "In the course of our conversations the President raised some of the major historical causes of difference between us, like belligerent rights, so-called fortified bases and so on, which are still active in forming opinion and we agreed mutually to examine them in the hope that we might arrive at understandings upon them" (*Commons Debates*, 5 November, 1929, c. 887). In the questions which followed Lloyd George was disposed to press the matter further and called for assurance that MacDonald had made no commitments on the issue of "belligerent rights." MacDonald reassured him on that point (*ibid.* col. 894). Stimson later expressed to Mr. Campbell, in the British embassy at Washington, his approval of MacDonald's' handling of the matter in the Commons (*Woodward*, 133).

[40] *U. S. 1929*, iii, 26; *Woodward*, 27.

[41] Hoover made use of what he called the Navy Board's formula for age and Admiral Jones' formula for gun power. This is about the closest approximation we get in the dispatches published to an actual application of a yardstick (*U. S. 1929*, iii, 26). Note that Hoover carefully refrains from using the word yardstick.

[42] *Woodward*, 107–108. It may be significant in this connection that no message from Stimson to Dawes about the Rapidan meeting is included in the U. S. documents Did nothing go? Dawes does not seem to have raised the question in his published correspondence or even in his *Journal*.

[43] *Ibid.* 107.

[44] *U. S. 1929*, i, 263.

[45] *Woodward*, 169, 185; *U. S. 1929*, i, 295, 305.

[46] *Woodward*, 126, 186; *U. S. 1929*, i, 304.

France that the British Labor Government was definitely less friendly than its Conservative predecessor had been.[47]

Nevertheless the French accepted the invitation to the Naval Conference. They felt constrained however, notwithstanding the efforts of both the British and the Americans to avoid anything like *à priori* commitments, not only to set forth their views on the subject at some length but actually to publish these views.[48] This French memorandum, transmitted on 20 December to the British government made the following points: (1) that programmes of disarmament should be based upon the League of Nations and not upon such unimplemented declarations of good will as the Kellogg Pact; (2) that naval disarmament could hardly be discussed apart from the general programme of disarmament; (3) that even naval disarmament should be a matter for all the powers, not merely for the five largest ones; (4) that in any programme of naval disarmament France would prefer to establish ratios in terms of global naval tonnage rather than tonnage by categories; (5) that she would attach great importance to any guarantees of security which might be established; (6) that she would look with favor upon a regional pact for the Mediterranean patterned after the Four Power Pact of 1922 for the Far East.[49]

This can hardly be taken to be an enthusiastic endorsement of the proposed conference, and it left to France many convenient doorways of escape if matters did not go to her liking. But the British in their published reply made the best of it, though they avoided any reference to guarantees of security. On the matter of a Mediterranean pact they were non-committal.

The British were indeed definitely opposed to a Mediterranean pact but were very anxious not to antagonize the French at this juncture. In the conference between Mr. Craigie of the British Foreign Office and M. Massigli for France late in December, Craigie brought forward apparently for the first time the idea that the U. S. was contemplating a cautious advance in the direction of implementing the Kellogg Pact by suggesting a general treaty for "consultation among the powers in the event of any power violating the Kellogg Pact." This seems to have been the official genesis of the so-called "Consultative pact" which occupied a great deal of attention later.[50] What Craigie apparently had in mind were some suggestions for implementing the Kellogg Pact which Hoover had proposed at Rapidan when Craigie was present.[51] Henderson in a dispatch to the British Ambassador at Washington some ten days later referred to the Rapidan conference and called attention to the fact that American newspapers were anticipating some proposals by the U. S. at the Naval Conference to implement the Kellogg Pact in some such way as Craigie had envisaged.[52] Henderson went on to say that any such move by the U. S. "would exercise a beneficial influence on the course of the Naval Conference." Henderson suggested that Mr. Stimson and Mr. MacDonald might dis-

[47] *Woodward*, 127; Tyrrell said that France regarded the Anglo-American rapprochement as a definite drawing away from France, *cf. Toynbee, Survey*, 1929, 31. The dispatches of Lord Tyrrell, English Ambassador at Paris, printed in *Woodward*, are particularly illuminating on French attitudes. *Cf.* particularly Tyrrell dispatches of 14 Oct., 16 Dec., 27 Dec., 30 Dec., 15 Jan., and the memo of Craigie's interview with Massigli; the memo. published by the French Govt. on the subject, dated 20 Dec., and published 26 Dec. and the British response to this memo. dated 10 Jan. and published 13 Jan. American dispatches on the subject are not very illuminating. There is nothing at all from U. S. representatives in France before 11 Dec. and that came from Armour, Counsellor of Embassy (*U. S. 1929*, i, 295). This was no doubt due largely to the fact that the new Ambassador, Walter Edge, had not yet arrived. His first printed dispatch is dated 14 Dec. (*U. S. 1929*, i, 297). It is interesting to observe that Tyrrell reports from "an absolutely reliable American source" a conference between Tardieu and Edge, 28 Dec. at which Hugh Gibson was present. (This is no doubt the luncheon referred to by Edge, *U. S. 1929*, i, 314). No dispatch from Edge on this interview is published in U. S. Documents, though from Tyrrell's account it must have been important.

[48] *Woodward*, 173, 173 n.

[49] France also said that she thought naval disarmament presupposed settlement of such questions as belligerent and neutral rights and the freedom of the seas. The British tried to get this omitted on the grounds that the initiative in the matter would probably be taken by the Americans. Subsequently Henderson retracted this statement (*Woodward*, 173; *cf.* also *Woodward*, 168). The British ignored this reference to freedom of the seas in their published reply to the French note. One wonders a little whether it was not raised deliberately by the French to create friction between the British and the Americans. It was apparently never alluded to again.

[50] *Woodward*, 167. René Massigli, Chief of the League of Nations Section of the French Foreign Office and French representative on the Preparatory Commission (*U. S. 1929*, i, 270 n.); at present (1947) French ambassador to Great Britain. Nicolson's statement in his *Life of Morrow*, 367, that Morrow himself "evolved the theory of a Consultative pact" is evidently inaccurate.

[51] It is noteworthy that Craigie himself drafted a statement on the subject at Rapidan for inclusion in the joint statement to the press. It was not included, but its general tenor was very much that conveyed to Massigli (*U. S. 1929*, iii, 31).

[52] *Woodward*, 183, a copy of this despatch was handed to Stimson 31 Dec. *U. S. 1929*, i, 316.

cuss the matter further when Stimson reached London.

Stimson in his reply expressed sympathy with the general idea, but (to quote the British Ambassador) "he said I am aware how suspicious certain elements in the Senate were of any even indirect implication that the U. S. might be entangled in European conflicts, and it would never do for the U. S. government to initiate any such proposal, which would at once be seized upon by critics in Congress in order to undermine the whole work of the Conference."[53]

The matter was not pressed further at this time.

The other major problem was that of Japan. Here the question really resolved itself into a question of ratios. Under the Washington treaty the 5–5–3 ratio had been applied as between Britain, the U. S. and Japan in the matter of battleships and aircraft carriers, made more attractive to Japan by the promise on the part of the Western powers not to increase fortification of their far eastern bases. Both the British and the Americans now wished to apply the Washington ratio to cruisers, destroyers, and submarines. Neither the destroyers nor the submarines presented many difficulties though Japan stood firm against the disposition of the Anglo-Saxon powers to do away with submarines altogether. The nub of the difficulty was the cruisers, or more specifically the 10,000 ton 8″ gun cruisers in which Japan was chiefly interested and where she claimed a 10–10–7 ratio with the power having the larger number of 8″ gun cruisers. A strict application of this ratio would mean, if the U. S. maintained its demand for twenty-one 8″ gun cruisers, that Japan would be allowed more of the type than Britain's fifteen. Even on the basis of eighteen for the U. S. the application of the 10–10–7 ratio would bring Japan's 8″ gun cruisers perilously close to the British quota.

This was the situation when MacDonald went to Washington, and it did not materially change before the Naval Conference assembled in London in January. Numerous conversations on the subject took place between MacDonald and Matsudaira, the Japanese Ambassador in London and between Stimson and Debuchi the Japanese Ambassador in Washington, but they all came to about the same thing, with Japan persisting in her demands. On the British side MacDonald attributed his opposition to the Japanese position to the Dominions who, he said, even talked of building additional cruisers on their own to redress the balance.[54] Stimson got the impression in November that the British were deliberately exploiting the Japanese demands as a lever to force down the U. S. demands. This fact does not appear in the U. S. Documents but it is abundantly clear in the correspondence between Henderson and Ronald Campbell who was temporarily in charge of the British Embassy in Washington.[55] On that account Stimson incessantly emphasized the importance of constant contact between the U. S. and Britain. On that account also he probably insisted upon keeping in his own hands in Washington the negotiations with Japan.[56] In conference with Campbell, Stimson intimated that perhaps the Japanese obduracy might be mitigated if Britain suspended any further development of her base at Singapore.[57] This should probably be taken simply as a register of Stimson's irritation at what he surmised to be double-dealing by the British. It was indignantly repudiated by Henderson [58] who at the same time disclaimed any intention of using the Japanese as a lever against the Americans. One suggestion did emerge from Stimson's conversations with the Japanese. It came out in his *aide memoire* to Debuchi of 12 November [59] in which he said, "It may well be that the word *ratio* will be an unfortunate word in the London conference. It may be possible that the eventual settlement will be made as a result of actual conditions in ships rather than ratios." Stimson reiterated the suggestion in his conversations with the Japanese delegates who passed through Washington in December en route for the London conference [60] adding that "he hoped that a basis for an understanding or agreement might be found in the light of what Japan had actually been doing in regard to her cruiser strength." [61] Actually, in the final outcome, adjustments made with Japan were made not by conceding her demands regarding the ratio, but by a deliberate slowing up of U. S. cruiser con-

[53] *Woodward*, 186. There is an interesting appraisal of the German attitude towards the Naval Conference from the British Ambassador in Berlin, printed in *Woodward*, 203.

[54] *U. S. 1929*, i, 214, 220.

[55] *Cf. Woodward*, 144.

[56] *U. S. 1929*, i, 289.

[57] *Woodward*, 145; Campbell said the idea was expressed by the Washington correspondent of the *New York Times* who attributed it to diplomatic circles.

[58] *Woodward*, 149.

[59] *U. S. 1929*, i, 274.

[60] *U. S. 1929*, i, 307, copy to British in *Woodward*, 189.

[61] *U. S. 1929*, i, 309.

struction in the final years to be covered by the agreement.[62]

III. THE LONDON NAVAL CONFERENCE

The London Naval Conference assembled on 21 January 1930. The delegates included for Britain the Prime Minister, Ramsay MacDonald, and the Foreign Secretary, Arthur Henderson; for the U. S. Henry L. Stimson, Secretary of State, Charles Dawes, U. S. Ambassador to Britain, David Reed, U. S. Senator, Joseph T. Robinson, Democratic leader in the Senate, and Dwight L. Morrow; for France, Aristide Briand, French Foreign Secretary, with André Tardieu, the French Premier, flitting in and out; for Italy, Dino Grandi, Italian Foreign Secretary; for the Japanese, Wakatsuki, head of the delegation and former Premier of Japan, Admiral Takarabe, Japanese Minister of Marine, and Matsudaira, Japanese Ambassador to Britain. There were others also, but these were the principal actors in the negotiations that followed.

The Conference met under the shadow of the great depression which followed the stock market crash in the U. S. late in the autumn of 1929. It met under the leadership of Ramsay MacDonald whose position as head of the British Government was an increasingly precarious one. MacDonald told Stimson at their first conference together "that he had never known any day whether or not before the day's session was over he might not find himself out of office." [63] Stimson himself felt that the British opposition would allow MacDonald to remain in office "on sufferance" until the conference was concluded but his position was certainly an insecure one and this sense of insecurity considerably restricted his freedom of action. The same thing was true of Tardieu and Briand, whose government actually suffered defeat during the conference and caused considerable delay. Even the Japanese attitude was affected by the February elections in Japan,[64] and the U. S. delegates were of course not only concerned about the probable reception of their decisions by the U. S. Senate, but also by the pressure of American public opinion. Indeed the only delegation which seems to have been relatively unperturbed by public opinion was the Italian one, and its attitude was a negative one throughout.

Two main problems occupied virtually all the attention of the Conference: (1) the effort to bring France in; (2) the effort to come to satisfactory terms with Japan.

The United States and the United Kingdom had not yet ironed out their differences when MacDonald left Washington in October. Sometime between then and the fourth of February they were ironed out. How this was ultimately achieved does not appear in the documents. Arthur Henderson had written to Howard, British Ambassador in Washington, on 20 December [65] suggesting that Stimson come over to London before the Conference and discuss the matter; yet in the long conference which Stimson had with MacDonald on 18 January [66] it was apparently never broached. Some time late in January or early in February the American delegation had a long conference with both the Japanese and the British on the subject,[67] and the outcome was a memorandum prepared by the American delegation in which they stated their programme. In this programme the basic contention between the U. S. and Britain on the question of 8″ gun cruisers was settled, as the British wished it to be settled, by assigning eighteen to the U. S., fifteen to Britain and twelve to the Japanese, and in general maintaining a 5–5–3 ratio in auxiliary vessels, as had been established for capital ships in the Washington Treaty of 1922. No mention was made in the memorandum of the French or Italian problems.[68] According to Mr. Stimson, the entire programme was accepted by the whole U. S. delegation and endorsed by Admiral Pratt, chief naval expert, with Admiral Jones dissenting. "All seven American delegates," Stimson added, "are now united in believing that the 21 cruiser programme could be insisted on only with great danger to the Conference's success." On February 5 Mr. Cotton cabled from Washington that the programme was heartily approved by Mr. Hoover. The following day Stimson issued a statement to the press in which the agreement with the British was outlined, though the proposed arrangement with the Japanese was omitted.[69]

This may be taken as a public announcement

[62] Stimson apparently felt that the Japanese problem was largely one of face saving. He said as much to Campbell in November, *Woodward*, 145; *cf.* also *Woodward*, 133.

[63] *U. S. 1930*, i, 2, 5.

[64] *U. S. 1930*, i, 24.

[65] *Woodward*, 171.

[66] *U. S. 1930*, i, 2. Years later Stimson spoke of MacDonald with great admiration and affection. Stimson and Bundy, *op. cit.*, 166.

[67] *U. S. 1930*, i, 13.

[68] Stimson explained at his press conference that these problems were not directly related to American ones, though their settlement was essential to the contemplated agreement. *U. S. 1930*, i, 20.

[69] *U. S. 1930*, i, 13, 18, 19.

that Britain and the U. S. had adjusted all their differences. Thereafter they worked together in complete harmony.

It will simplify a complicated situation if we proceed first to a consideration of the Japanese problem, which was settled first but which of course ran *pari passu* with the French problem.[70]

The U. S. seems to have taken the chief responsibility for dealing with Japan, the British, with France. On the American side, Senator Reed was the active agent in conversations with the Japanese, and the final agreement with Japan was largely the work of Reed for the U. S. and Matsudaira, Japanese Ambassador to England, for the Japanese.

From the first the Japanese stuck out valiantly for a 10–10–7 ratio in auxiliary vessels, particularly in 8″ gun cruisers, and in relation to the larger allotment in that category agreed upon between the U. S. and Britain. They were apparently disposed to be more accommodating in the matter of smaller cruisers. The British had all along taken the stand that they could not concede more than twelve 8″ gun cruisers to Japan and still keep within the limits of the Anglo-American agreement. Both the Americans and the British attached much more importance to maintaining the Washington Treaty ratio, 10–10–6, in 8″ gun cruisers than in any other category of auxiliary vessels. The reason for this appears to have been that these larger cruisers were to a much greater degree weapons of offensive warfare than the other categories. In Japan the navy interest took a very resolute stand and they seem to have succeeded in creating a strong public opinion behind them.[71] Actually the negotiations marked time until after the Japanese elections of 20 February, which resulted in a decisive victory for the party in power.[72] This served to strengthen the hand of the Japanese delegation, but not to diminish their demands.

The way out of the impasse may have originated with Mr. Cotton, acting U. S. Secretary of State in Washington, who suggested, in a letter to Stimson of 18 February, that the cruiser building programme of the U. S. might be slowed up, so that, while it had eighteen 8″ gun cruisers in blue print, only fifteen of these would be completed when the term of the treaty was reached in 1936. Reed and Matsudaira had arranged a compromise in these terms by the end of February but it had yet to be accepted by the other Japanese delegates and of course by Tokyo. Reed, in transmitting to Castle at Tokyo an account of the negotiations, pointed out that if Japan should still insist upon a flat 10–10–7 ratio the result could only be to disrupt the Conference. He said that the British acquiesced in the compromise and that Matsudaira realized that it was as far as the U. S. would go, but that the Japanese naval men in their delegation thought that by standing out they could get more.[73] Reed's view of the matter does not seem to have been supported resolutely by the State Department at Washington, which revealed some disposition to yield to Japanese demands; even Borah appears to have been in an accommodating mood. But Stimson thought the U. S. should stand fast. Two days later, 10 March, he wrote, "The negotiations with the Japanese are very tedious, as the Japanese, evidently in an endeavour to satisfy internal dissentions in their delegation, are bringing to us recurrent propositions which they know we will refuse. However we believe we are slowly reaching a point of agreement with them which will be satisfactory." But the State Department, prodded probably by Mr. Hoover, was impatient and favored further concessions to Japan.[74] In the course of the next few days Stimson conceded to the Japanese the 10–10–7 ratio in 6″ gun cruisers and parity in submarines.

The situation was rendered more critical by the defeat of MacDonald in an important vote in the House of Commons on the evening of March 11, and though he did not resign Stimson feared that he might not long survive. "I have come to the conclusion," he wrote,

> that both for the reason of this possible eventuality . . . and as a tactical manoevre against the delays which we are suffering from Japanese and French sources, we should proceed therefore with the drafting of a two power treaty with Britain. I discussed the matter with MacDonald this morning and he agreed with me. . . . Consequently I had a conference

[70] There is some evidence to support the belief that France was urging Japan to stick to her demands with the idea of sabotaging the Conference. *U. S. 1930*, i, 25, 58, 67; *Woodward*, 251, 265 n.; the British definitely charged the French with this. *Woodward*, 265 n.

[71] Henderson wrote to Tilley, 15 November, that this sentiment was the result of determined naval propaganda. *Woodward*, 248.

[72] Castle, U. S. Ambassador to Tokyo, suggested 25 Jan. that Japan would forget the 10–10–7 ratio if assured that the U. S. had no plans for China which might lead to war, *U. S. 1930*, i, 10. Castle himself was disposed to give some such assurance, *ibid.* i, 11, but the State Dept. warned him to avoid any suggestion that Japan and the U. S. had any common policy regarding China, *ibid.* i, 12.

[73] *U. S. 1930*, i, 31, 43.

[74] *Cf.* Cotton to Stimson, *U. S. 1930*, i, 46, 48, 52, 56, 59.

with Waketsuki . . . I told him that the adverse vote in the House of Commons last night had troubled me greatly and that I was proceeding to close up with MacDonald.

Stimson passed on the word to Castle to Tokyo the following day and the threat of a two power pact seems to have turned the trick. Stimson at least thought so, and on the day following announced [75] that an agreement had been reached with the Japanese which commanded the support of the whole U. S. delegation and of a majority of the Japanese delegation. By its terms Japan received 60 per cent in 8″ gun cruisers, 70 per cent in 6″ gun cruisers, 70 per cent in destroyers and parity in submarines, with delayed construction of three U. S. 8″ gun cruisers. In this proposal the British concurred. "The above compromise," Henderson wrote to Tilley, British Ambassador at Tokyo, "represents a considerable departure from our original attitude, and during the negotiations we have made concession after concession to Japan before reaching the present position. It is useless for the Japanese government or the Japanese delegation here to think that we can go any further." [76]

Henderson instructed Tilley to keep in touch with Castle in Tokyo, but to avoid creating the impression of concerted pressure. After the middle of March the Japanese problem took the form of winning the support of the Japanese government to the compromise. We can follow the course of events in Tokyo from the dispatches of Castle to the State Department and of Tilley to the Foreign Office, though some important dispatches from Tilley have been omitted in Woodward.[77] Castle's reports indicate that the big Navy party in Tokyo was representing the proposed compromise in the Japanese press as an American plan, which it then proceeded to denounce. The conflict was really between the Japanese Navy and their Foreign Office. Tilley reported strong hostility to the U. S.,[78] and found the Foreign Minister, Shidihara, indignant that Britain should once again be forcing her old friend Japan to give way to the U. S. Tilley added, "Therefore the less I see of the U. S. Ambassador the better." Henderson in London took a dim view of the situation. "It is evident," he wrote to Tilley on 19 March, "that a determined effort is being made by Japanese naval authorities to reject the compromise and nothing should be left undone to prevent such a disaster from occurring." Henderson said that the Japanese delegation in London was unanimous for the compromise. He instructed Tilley to seek an interview with the Japanese Prime Minister and insisted that if Japan did not agree the era of naval competition in the far east would recommence.[79] Campbell, however, at the British Embassy at Washington found the State Department reasonably confident of the outcome. Castle reported on 20 March that the Japanese naval men were proposing a Japanese-American agreement on China.[80] This the State Department resolutely stamped on and wrote to Castle that if the present compromise were rejected the alternative was the U. S. twenty-three cruiser programme. But the situation changed for the better presently. On the twenty-fifth Stimson wrote that cables from Tokyo indicated probable approval,[81] and finally on 2 April the Japanese delegation reported a complete acceptance from Tokyo. Some minor points remained to be ironed out, but actually the Japanese problem was solved;[82] and the peace treaty as signed later in the month virtually embodied the terms of the compromise.

IV. THE FRENCH PROBLEM

In the case of the French as of the Japanese problem it must be borne in mind that the newly available source material is from American and British archives. On that account it inevitably reflects the French problem as seen through American and British eyes. As has been observed already, the Americans were only interested in France so far as her attitude affected Britain. They were disposed to discount the French naval strength, however great were French pretensions. Borah's attitude in this regard is probably char-

[75] *U. S. 1930,* i, 58, 60, 62.

[76] *U. S. 1930,* i, 61, 78. Henderson's account of the terms in his dispatch to Tilley of 15 March, is illuminating, *Woodward,* 249.

[77] So, for that matter, were some of Castle's from the *U. S. Documents; cf. Woodward,* 249 n. i, and note 81 *infra.*

[78] *U. S. 1930,* i, 66; *Woodward,* 266.

[79] *Woodward,* 262.

[80] *U. S. 1930,* i, 69.

[81] These cables do not appear either in *U. S. Documents* or in *Woodward.*

[82] How the favourable results were finally achieved in Tokyo is not revealed in the available material. The issue turned upon internal politics. *Cf. Toynbee, Survey, 1930,* 80–81. It is to be hoped that Tilley and Castle kept their governments better informed about the situation in Tokyo than appears from the published dispatches. If the dispatches published by the State Dept. and the Foreign Office from Tokyo at this juncture are anything like complete they reveal an amazing lack of adequate information both on the part of William Castle and of Sir John Tilley.

acteristic. He remarked that the tonnage demands of France were impossible and that France was behaving like a spoiled child. Since he did not believe that France would build, he did not care how much they asserted their right to build.[83] The British were naturally much more concerned about the French naval programme. It was axiomatic in British naval circles that British naval strength should be equal to that of any two European powers. With Germany out and Spain and Russia negligible, this meant in plain terms that the British navy should be equal to the strength of the French and Italian navies combined. Italy under Mussolini was already beginning to talk about the Mediterranean as her sea, and was, on paper at any rate, planning a navy aimed at parity with France. At the moment French naval strength was far superior to Italian. France insisted that it should remain so. She was satisfied with the *status quo* and was not prepared to make continuing commitments except on the basis of a naval tonnage at least 240,000 tons in excess of Italy. Italy refused to accept any other guiding principle than parity. From the beginning Italian and French aspirations clashed head on. Realists like Dwight Morrow urged that both countries forget theoretical ratios and come down to the practical question of ships built, building, and planned.[84] But Italy refused to commit herself to a concrete programme. She simply demanded parity with any continental European power, and rather unctuously expressed her willingness to accept parity on the most modest terms. But parity she would have. France argued that since Italy had few colonies and since her naval problems were limited to Mediterranean waters she did not need anything like the naval strength which France, with shore lines on the Mediterranean and on the Atlantic, and with a colonial empire extending from South America on one side to Indo-China on the other, had to command. France indeed maintained that her naval strength in the Mediterranean alone should be at least equal to that of Italy. Actually the French were considerably concerned about the increasingly flamboyant, imperial pretensions of Mussolini and much irritated by Italian pressure along the whole North African shore and particularly in Tunis.

Anything like competitive naval building between France and Italy meant that English naval strength must be increased *pari passu* if the two power standard was to be maintained. In that case of course the tentative agreement between Britain and the U. S. fell to the ground.

This situation explains why it was that France demanded from Britain, as the price of a reduced naval programme, a regional pact covering the Mediterranean which would furnish a guarantee of the *status quo* there.

France was moreover always reluctant to deal with naval disarmament separately from the whole question of disarmament. She recognized that Britain was little interested in land armament, and she inclined to the belief that the way to win British support for her programme of land armament was by trading it off against naval armament in something like the way that had been accomplished in the discredited Anglo-French naval conversations of 1928. Otherwise the French felt that when Britain had achieved naval disarmament on her own terms she would then proceed to impose land disarmament upon France.

Another factor in French calculations was her insistence upon the reference of all international problems to the League of Nations. Her initial attitude at London was that all agreements there should simply take the form of recommendations to the Preparatory Commission of the League of Nations and should not themselves be binding. Later Tardieu appears to have abandoned this position,[85] but it was indicative.

Harold Nicolson in his *Life of Dwight Morrow* maintains that the French delegation came to London "with obstinate determination to wreck the conference." Stimson at the outset did not think so. Neither did Morrow,[86] but certainly the French were very suspicious of the British, and were fearful that MacDonald's visit to America indicated a reorientation of British policy and a withdrawal from continental affairs.[87] MacDonald himself seems to have been definitely anti-French. Though he got on famously with Briand, he was temperamentally completely out of tune with Tardieu.[88] Arthur Henderson, the British Foreign Secretary, was much more sympathetic with the French, and the permanent officials of the British Foreign Office, "who realize," Stimson wrote, "that they must continue to have France as their near neighbor, seem to share Henderson's

[83] *U. S. 1930*, i, 49.

[84] Sir Maurice Hankey, secretary of the British delegation, thought this the way out, *U. S. 1930*, i, 6.

[85] *U. S. 1930*, i, 5.

[86] *Nicolson, op. cit.*, 356; *U. S. 1930*, i, 26.

[87] *Woodward*, 127.

[88] *U. S. 1930*, i, 77.

feeling." [89] Probably MacDonald came nearer to reflecting British public opinion. Stimson found that Lloyd George, the Liberal leader, and Baldwin and Churchill, the Conservative leaders, were definitely anti-French.[90]

Negotiations with the French were largely in British hands, and much of the published record of them is in the form of conversations between Craigie of the British Foreign Office and René Massigli of the French Foreign Office, who was Briand's right hand man.[91] The problem was to get from France an agreement to reduce her naval programme. The French declined to consider this unless Italy was prepared to make commitments or, failing that, unless the British were willing, by a Mediterranean pact, to join the interested powers in a guarantee of the Mediterranean *status quo*. The British declined to go so far as that but suggested instead a consultative Mediterranean pact patterned after the four power Pacific treaty of 1922.[92] Already Craigie had suggested the possibility of drawing the U. S. into some such pact and Massigli in his conference with Craigie on 13 February thought this might be a way out.[93] In mid-February both Stimson and Morrow were definitely hopeful that the French and British would come to terms, and Stimson felt that the delay was mainly due to MacDonald's preoccupation with many things and to Tardieu's frequent absences in Paris. Actually, of course, Tardieu's government was tottering to a fall. With its fall in mid-February the note of optimism in Stimson's letters gave way to one of pessimism. At the State Department in Washington sentiment in favor of a three power pact increased, particularly in view of a developing pressure upon the President to give France some sort of assurance of political security.[94]

This pressure seems to have originated among American press correspondents in Paris, of whom Mowrer of the *Chicago Daily News* and James of the *New York Times* were the most conspicuous. It naturally had the sympathetic support of Briand and was, in the opinion of the State Department, French propaganda. It was taken up vigorously by the Foreign Policy Association. The opinion expressed was that action of the sort demanded of Mr. Hoover would alone save the conference. Hoover himself and the State Department with him were definitely of the opinion that though such action might save the conference it would destroy the last hope of any ratification by the U. S. Senate.[95]

Early in March Stimson himself seems to have felt that unless something were done to conciliate the French, and to induce them to reduce their programme of naval building, MacDonald might find it impossible to accept the limitations on British naval construction already tentatively arrived at in the Anglo-American conversations. This would mean the end not only of a five power pact but even of a two power pact. Stimson told the French plainly that the U. S. would not be a party to any pact, consultative or otherwise on the Mediterranean, but he held out hope that something might be offered in the way of an amplification of the Kellogg Pact. In conversation with Briand on the subject he expressed himself as willing to discuss it—after naval negotiations had been worked out. In his observations to the State Department, Stimson took the position that it deserved careful consideration if the conference could be saved no other way. He even went so far as to prepare a draft of a treaty incorporating the idea of a consultative pact [96] which he showed both to MacDonald and to Tardieu. "They are," he wrote, "inclined to favor it."

Mr. Hoover's reply was distinctly unfavorable. "The entanglement," he wrote, "of political guarantees in those negotiations, though they may be indirect, is more dangerous than anything else to the whole American acceptance of results." [97]

We are almost entirely dependent upon the American dispatches for the ebb and flow of opinion about the French negotiations. This is partly because Stimson had to maintain contact with his government by dispatches whereas MacDonald and his colleagues maintained their contacts orally. If we had any record of British Cabinet discussions it might be very illuminating, but Professor Woodward's documents reveal almost nothing of what was going on behind the facade of more or less formal conferences. Even the American dispatches do not cover adequately Reed's separate dealings with Matsudaira on the Japanese problem, or Dwight Morrow's valiant efforts to keep the Anglo-French negotiations alive. Harold

[89] *Cf.* Stimson's excellent analysis of the situation in *U. S. 1930*, i, 76.

[90] *U. S. 1930*, i, 29.

[91] Both the British and the French seem to have made much greater use of professional Foreign Office officials than did the U. S.

[92] *U. S. 1930*, i, 36.

[93] *Woodward*, 209.

[94] *U. S. 1930*, i, 26, 29, 32.

[95] *U. S. 1930*, i, 40.

[96] *U. S. 1930*, i, 36, 39.

[97] *U. S. 1930*, i, 41.

Nicolson, in his *Life of Morrow*, cites Edgar Mowrer and Sir Arthur Salter to the effect that without Morrow the Naval Conference could not have reached an agreement.[98] This view of the matter is hardly sustained by the documents. Stimson clearly depended upon Morrow on the French side just as he depended upon Reed on the Japanese side. But the most that can be said is that Morrow probably prevented the French from withdrawing altogether. As it was their participation in the final treaty was peripheral as was that of the Italians. The effort to persuade them to consent to any general programme of naval reduction was a flat failure.

The outcome of Hoover's opposition to a consultative pact was that Stimson, who appears to have been sympathetic with the French position, swung quite around and MacDonald apparently as well. When Briand (8 March) opened up again the question of an amendment to the Kellogg Pact, Stimson was not encouraging, and two days later Briand told Stimson and Morrow that the matter was ended so far as he was concerned.[99] At the same time MacDonald made it quite clear to Briand that Britain would enter into no consultative pact implying military aid, and he emphasized his position by a broadcast to the U. S. and Canada, 9 March, in which he said, "We shall not agree to any treaty which may result in an entangling military alliance." [100] But Stimson still hoped that some formula might be worked out, perhaps along the lines of the Rapidan statement. In any case he believed that no amendment of the Kellogg Pact would help the French attitude, unless they were allowed to read into it the implication of military aid.

On 12 March the British and French delegates conferred together and got nowhere. Stimson sat in on the meeting and remarked that the atmosphere was very tense. Sentiment in the conference at large was correspondingly pessimistic. Briand himself issued a statement which seemed to close the doors to a substantial five power pact.[101]

The crisis appears to have been reached with the receipt of a dispatch from Sir William Tyrrell, British Ambassador at Paris, on 22 March, the essential part of which ran as follows:

> 1. The French believe that their delegation cannot do anything more in London so long as France is being asked by Great Britain to lower her figures without entering into a Mediterranean agreement with her and so long as Italy's demand for parity continues.
>
> 2. Therefore, if the British have decided to refuse a Mediterranean agreement and can do nothing more with the Italians, and also will agree to let the French figures stand as they are, the French will help wind up the conference with a report to the League which would contain certain agreements with respect to the regulation of reduction, naval holiday for certain construction, methods of limiting naval armament, and so forth.[102]

Actually this followed reasonably closely the solution which was ultimately reached. Stimson himself thought that it marked the "end of the attempt to secure a five power agreement," and that MacDonald would "try for a smaller one." [103]

It was to face this crisis [104] that the U. S. and the British delegates conferred together on the twenty-fourth of March. In the course of the conference MacDonald reported on a private conversation he had held with Briand five days earlier on the general question of political pacts. He had told Briand at the time that England would not add to her existing military commitments but "would cooperate in any practical effective way for strengthening the machinery for keeping the peace," and if war came would stand by her obligations under Article XVI of the League Covenant. Briand knew of course that no imposition of sanctions by the League against a declared aggressor could be achieved without the supporting vote of Britain in the League Council, but he raised the question as to whether Britain, even if she voted in favor of sanctions, would support them by appropriate military action. MacDonald had hastened to reassure Briand on that point and had called his attention to the British affirmation of her

[98] Nicolson, *op. cit.*, 357. Mr. Stimson (Stimson and Bundy, *op. cit.*, 169–172) does not accord to Morrow the preeminent place which Nicolson ascribes to him in the French negotiations.

[99] *U. S. 1930*, i, 52. Briand told Stimson later that he had never expected the U. S. to yield to a consultative pact "but had made application because he had been directed to do so," *U. S. 1930*, i, 63. Briand, possibly encouraged by Mowrer and James, told the British that he thought the American position was illogical and evidently was more optimistic about a favorable reaction from the U. S. Senate than Hoover was. *Woodward*, 238.

[100] *U. S. 1930*, i, 55; Dawes, *Journal*, 167.

[101] *Woodward*, 242; *U. S. 1930*, i, 63.

[102] This extract is from Stimson's dispatch of 22 March. *U. S. 1930*, i, 72. Strangely enough Tyrrell's dispatch is not printed in *Woodward*.

[103] *U. S. 1930*, i, 72.

[104] Stimson said later (*U S. 1930*, i, 94) "that the conference was on the brink of a precipice . . . was recognized by all parties."

obligations in this regard in a note to Germany in 1925 which had been formally incorporated, as Annex F., in the Locarno Treaty.[105]

All this MacDonald recited to the assembled British and American delegates. But he confessed that British obligations under the League involved dangerous potentialities of conflict with the U. S. This would be notably so in the case of a sanction which took the form of a blockade. Suppose American traders refused to conform to the blockade. What then?

Here indeed was the basic factor underlying the efforts to get the consent of the United States to a consultative pact. MacDonald never expected and probably Briand never expected that the U. S. would underwrite French security. But what MacDonald at least hoped for was that the U. S. could be drawn into conference upon issues threatening the Kellogg Pact, so as to safeguard Britain against possible conflict with the U. S., in the event that Britain, as the one great naval power in the League, should be constrained to impose sanctions by blockade.

Against this background Stimson's statement at the conference regarding the American attitude towards a consultative pact, though it constituted little more than a reiteration of what he had said previously, takes on a new significance. "If," he said, "the other nations would take care of the question of mutual assistance in such a way as to secure a substantial reduction of armament, I should think it possible for us to consider, with an open mind, the question of a provision for consultation among the signatories to our agreement, with explicit denial of military action." [106]

What it actually worked out to was a pattern in which Britain, secure in the knowledge that she could count upon American participation in the consideration of international crises as they arose, felt much more disposed to emphasize her commitments under the League.[107]

Arthur Henderson seized upon the opportunity at once and succeeded in persuading MacDonald to reconsider the British position towards France. According to Stimson, he got authority from MacDonald to telephone Briand that the British were ready to discuss the subject of mutual security. The result was that Briand returned to London. He told Stimson later that America "had given the push that broke the jam between the British and the French." "The trend for the last twenty-four hours," Stimson reported on 26 March, "has been more encouraging than anything we have experienced for a long time." At lunch with Lloyd George, he found the mercurial Welshman really hopeful. "His statement," Stimson added, "is significant, as his coalition is a big factor in keeping MacDonald in office." [108]

Lord Tyrrell was considerably encouraged by Briand's speech in Paris on 25 March. "I cannot too strongly emphasize," he wrote, "that in the mind of the French government what is at issue is not the use of military force against the aggressor, but that of an undertaking to prevent war and thus make the use of force impossible and unnecessary." [109]

On the 27 March [110] Henderson and Briand, in conference together discussed at length the question of clarifying the application of Article xvi of

[105] Nicolson in his *Life of Morrow* (p. 371) says that Morrow unearthed this Annex F. with the help of Sir Robert Cecil and Mr. Noel Baker. MacDonald said the matter was drawn to his attention at a meeting of the British delegation on the morning of 24 March. One suspects that Cecil and Noel Baker were the real authors of the idea—and that Morrow, like MacDonald, seized upon it.

[106] *U. S. 1930*, i, 800, which is Stimson's own account. The British account (*Woodward*, 272) is to the same effect.

[107] The conference had hardly terminated before private cables were once more on their way to American peace societies to the effect that members of the American delegation believed that an affirmative statement from Mr. Hoover on the question of the pact would smooth out all difficulties (*U. S. 1930*, i, 81). It is hard not to sympathize with Mowrer and others in their efforts to win some commitments from the U. S. to the general problem of European security, but certainly the methods used were dangerous and might very well have defeated their own ends. Mr. Hoover was probably right in his belief that any commitments of the sort indicated would simply have killed the treaty in the Senate. His personal message to Briand on the subject of American attitudes is very illuminating,—"The American mind has not come to the point of accepting any general plan of methodical procedure for the pacific settlement of international disputes and particularly, it is not ready to commit itself to any plan in cases of violation of the Pact of Paris or as to action in cases of aggression. But the American mind is, I think, ready to take up and consider and, I believe, approve certain immediate and obviously practicable steps which will do much to obviate and to remove the source of international controversies and thus help prevent war. . . . Success in such practical steps, one by one, seems the way the American people are prepared to accept more systematic or automatic methods of procedure of international cooperation. . . . Recognizing the realities of our situation, however, this cooperation can best be developed, as I have said, by dealing with limited and positive questions as they may arise" (*U. S. 1930*, i, 97).

[108] *U. S. 1930*, i, 84.

[109] *Woodward*, 277n.

[110] *Woodward*, 276.

the League. From the record of their conversation it appears that the French had already submitted a formula on the subject which had seemed to the British excessive and that the British had presented a counter-draft which had seemed to the French inadequate.[111] Briand advocated some instrument of mutual assurance, "not directed against any particular power and nothing which would resemble in any sense the old alliances or pacts of pre-war days." But Henderson took the view that what the French really wanted was a satisfactory assurance that Britain would carry out loyally her obligations under the League. The outcome was that the matter of drafting a mutually acceptable statement was referred to subordinates, with Massigli acting for the French and Malkin acting for the British.

All this was promising enough. But three days later MacDonald issued a statement to the press to the effect that "any further military or naval commitments are impossible, for that would be tantamount to tying ourselves down to military operations without being able to control the situation from which they have arisen. No British government could undertake such commitments, which would be contrary to the whole feeling of the British people." [112]

The interpretation of this statement in Washington was that the hope of a five power pact had been abandoned. MacDonald evidently did not intend to convey that impression, but his tactics at this juncture are a little hard to follow. In his dealings with the French he was apparently anxious to convince them that England was already bound under Article xvi of the League to come to their assistance against any aggressor. And yet in his broadcast of 9 March and again in his statement to the press just cited he was emphatic upon the point that Britain would make no further military commitments. Probably his public pronouncements were designed to conciliate isolationist trends in British public opinion. Nevertheless what he said *in camera* to Briand must have lost a good deal of its force when viewed in the light of what he was saying out loud.

Two days after the press statement Craigie and Massigli were conferring again and MacDonald had apparently instructed Craigie to find out what reduction France was prepared to make in her naval estimates in view of the concessions which the British were offering and the prospect of a consultative agreement with the U. S. Craigie put the question to Massigli in these terms, pointing out at the same time that the Japanese were apparently ready to come to terms and that a three power agreement was in sight if a five power agreement could not be reached. Massigli raised the Italian difficulty. He raised also the old issue of the *quid pro quo* on the question of land armaments. Upon the latter question Craigie intimated that the British would probably be accommodating. But one gets the impression that Craigie was presenting the French with something in the nature of an ultimatum.

Nevertheless MacDonald and Stimson were still hopeful of the outcome,[113] and Briand told the Americans that "he considered the two nations very close together."

On April 4 Stimson reported that Anglo-French negotiations were going steadily forward, but he was less sanguine the following day. "MacDonald's mind I can see," he wrote, "is influenced very much by the evident reaction in British opinion against further political commitments, of whatever nature, in the European situation." [114]

The British documents as printed in Woodward are strangely silent upon the march of events during the first week in April. Probably the deciding factor was MacDonald himself, who seems to have oscillated between the idealistic pull of Henderson —the old MacDonald of the Geneva Protocol— and the pull of political realities—the new MacDonald, with an eye to the main chance, who was already making friends of the mammon of unrighteousness and who, a little more than a year later, was to break sharply with Henderson and many of his old Labor friends, and to lead a government recruited for the most part from the camp of his whilom enemies.[115]

In any event, Briand returned to Paris on 5 April to consult with his government, taking with him a proposed statement designed to clarify the attitude of Britain towards the implementation of her obligations under Article xvi of the League. MacDonald laid the whole matter before his Cabinet on April 7.

[111] Neither of these is printed in *Woodward*.

[112] *U. S. 1930*, i, 98n.

[113] *U. S. 1930*, i, 95.

[114] *U. S. 1930*, i, 102, 103.

[115] The description of MacDonald's vacillations as set forth by André Geraud (Pertinax) in his article on the London Naval Conference in *Foreign Affairs* for July 1930 (518 ff.) is hardly substantiated in the documents. Pertinax however probably does reflect an attitude towards MacDonald which was widespread in French official circles.

On April 8, with Briand back again from Paris, the British and the French delegates reassembled.[116] Briand announced that the French government was disappointed with the prepared statement. MacDonald in turn reported that the Cabinet insisted that any implementation by Britain of her obligations under Article xvi of the League was subject to the approval of Parliament.[117] Briand's response to this was that it left the existing situation essentially unchanged. There was some talk of a four power pact, based upon figures designed to bring naval forces of the U. S., Britain, Japan, and France into equilibrium, with an escalator clause to protect France against Italian competitive building. But neither Briand nor MacDonald was very sympathetic with the idea. They adjourned however to meet the following day. MacDonald told Stimson that the French were very conciliatory and were evidently anxious for an agreement. But Stimson observed that MacDonald was in fact very despondent and very tired.[118]

The conference on the following day produced little more than insistence by the French that the disparity of 240,000 tons between their navy and Italy's must be maintained. Briand was even unwilling to make any attempt to bring French and British figures into equilibrium without reference to Italy. And so the meeting adjourned again, and Briand and MacDonald both undertook to consult with Dino Grandi, the head of the Italian delegation.

In a third session the following day a letter was read from Grandi which was quite uncompromising.[119] Italy would undertake not to build beyond naval parity with France. But she would go no further. The situation was obviously hopeless. Very likely French emphasis upon the Italian issue was simply an attempt to place the blame for the failure to come to terms upon the Italians.

On the afternoon of the same day (April 10) Stimson and MacDonald, "in order to forestall any acrimonious termination of the conference," took up the preparation of a composite treaty. Stimson and Morrow had already prepared a skeleton outline of such a treaty, the essence of which was that all five powers should agree to a holiday in the building of capital ships, should acknowledge certain humane obligations in submarine warfare and should accept certain standards in the replacement and scrapping of vessels of war; and that England, the U. S., and Japan should accept the limitations upon auxiliary naval tonnage already agreed upon.[120]

Briand and Grandi both approved and so, at 6:30 P.M. on 10 April it was decided. The same evening MacDonald with the approval of all the delegations made a statement in the House of Commons.

> After announcing that an agreement had been reached between Great Britain, the United States and Japan on a naval programme including all categories of ships, Mr. MacDonald continued as follows: "The differences in the naval requirements of France and Italy have proved to be intricate, and have not so far been resolved. At a meeting early this evening between the French, the Italian and the United Kingdom delegations, it was decided that, as such substantial agreement had been come to, it was thought unnecessary and undesirable to keep the full body of delegates sitting in London pending a settlement of difficulties which primarily concern these three delegations only; and so we shall propose, at a plenary session to be held early next week, that the agreements now come to shall be signed, and the Conference adjourned, on the understanding that France, Italy and ourselves shall continue efforts to come to an agreement in unison with that which has been reached between the United States, Japan and ourselves." [121]

Twelve days later Stimson cabled Hoover that the treaty had been signed. "The form," he said, "is satisfactory and the spirit of the occasion excellent." [122]

The text of the treaty is printed both in *Woodward* and in the *U. S. Documents on Foreign Relations*. It is a familiar document and calls for no detailed comment. In the long view the best thing that can be said for it is that it did bring about a temporary cessation of competitive naval building among the great naval powers. Probably it did something to strength Anglo-American solidarity.[123] At least it mitigated the ill feelings between

[116] *U. S. 1930*, i, 103; *Woodward*, 293.

[117] The draft agreement modified to meet the objections of the British Cabinet is printed in *Woodward*, 300.

[118] *U. S. 1930*, i, 104.

[119] Printed in *Woodward*, 308–309.

[120] *U. S. 1930*, i, 105.

[121] *Woodward*, 310–11, citing *Commons Debates*, 10 April, 1930; c. 2473.

[122] *U. S. 1930*, i, 107. The text of the treaty is printed in *U. S. 1930*, i, 107–125, also in *Woodward*, 589–597. For its ratification, *cf. U. S. 1930*, i, 107, n. 25. Subsequent negotiations between France and Italy, envisaged in MacDonald's announcement to the House of Commons, were begun almost at once. Documents relative to these negotiations are printed in *Woodward*, Second Series, i, 354–472, and ii, 383–434.

[123] Mr. Stimson, in appraising the significance of the treaty later, made much of this point. He also believed

the two countries engendered by the abortive Geneva Naval Conference of 1927 and the discredited Anglo-French Naval Conversations of 1928. Those who were hopeful that the treaty marked the beginning of the emergence of the U. S. from her isolationism were however doomed to disappointment. And those who believed that it presaged a progressive trend towards disarmament by land and sea were equally disappointed. Of the men who played a major part in the negotiations Mr. Hoover and Mr. Stimson alone survive. It is very satisfactory to record that both of them are still fighting vigorously for the cause of international peace.

that the treaty did something to improve relations between the U. S. and Japan (Stimson and Bundy, *op. cit.* 173).

WHY DID THOMAS PAINE WRITE ON THE BANK?

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PAINE defended the Bank of North America first of all because he believed in the principles on which it was established and felt sure that its influence upon the future of the United States would be beneficial. The truth of this is incontestable. New evidence now reveals to us in addition the particular circumstances which led Paine to announce publicly his adherence to the Bank. This evidence is found in a letter by Paine in the *Pennsylvania Gazette,* December 21, 1785, which so far as I know has never been reprinted or discussed. This letter reveals that Paine's first public statement was elicited because other published works on the contrary side had been attributed to him and that intense personal embarrassment made him reluctant to criticize the Assembly for acting against the Bank. His embarrassment stemmed from the fact that the Assembly which he was criticizing had just voted him a payment of £500.[1] More important, the letter shows that the arguments in defense of the bank in Paine's later *Dissertations on Government; the Affairs of the Bank; and Paper Money* (1786), arguments which some recent critics have characterized as "conservative," were not actually founded upon conservative attitudes in Paine's own mind. By means of this letter we see that at first his principal objections to the anti-bank faction were political, not economic. This evidence should help to check the drift of recent scholarship, which has emphasized economic interpretations and neglected political.

A brief summary of the controversy over the bank will prepare for a discussion of Paine's letter.[2] In 1781 the Bank of North America founded by Robert Morris, was granted a charter by both the Continental Congress and the State of Pennsylvania. Two years later when the Revolution came to an end, land values dropped and a great scarcity of money ensued. The farmers and mechanics, who suffered most of all, felt that their economic plight could be remedied by the issuance of a plentiful supply of paper money. The Bank of America stood in the way of this plan, for if the bank refused to accept the new money on the same terms as specie, the new currency would be almost worthless from the outset. When the directors and stockholders of the bank proceeded to issue a petition against paper money, public sentiment rose up against the bank. The Assembly immediately authorized the issuance of bills of credit to the value of 150 thousand pounds and welcomed a petition accusing the bank of dangerous tendencies and demanding the repeal of its charter. Although the bank would still have a charter from Congress if the state charter were to be taken away, the low prestige of Congress at this time made the Congressional charter almost worthless. A committee of inquiry appointed by the legislature to investigate the bank—a committee which Paine condemned for "making no inquiry at all"[3]— denounced the bank on the grounds that it sent most of the specie out of the country in the form of dividends to foreign shareholders and concentrated the rest in the hands of its domestic stockholders. As a result, a bill to repeal the charter passed a second reading in the House on April 4, 1785, and on September 13 the charter was repealed.

The action of the Assembly came just short of dealing the mortal blow. Deposits shrank and European investors berated American bad faith. The bank limped on under its Congressional charter, however, and on February 2, 1786, it was given a charter by the state of Delaware. This gave the bank new life, and Robert Morris began a struggle to win back the Pennsylvania charter. On February 18, 1786, Paine brought out his *Dissertations on Government.* This pamphlet has been a puzzle to students of Paine, for previously Paine had

[1] This was not an extraordinary act of generosity. In the previous year the Senate of New York had presented him with a choice of two farms as a tribute to his services to the United States. See: *Pennsylvania Packet,* April 27, 1784. The Pennsylvania Assembly, furthermore, was somewhat dilatory in making its acknowledgment. On December 22, 1789, a letter by Paine was read to the committee appointed to consider his eminent services in which Paine expressed willingness to have the question of his compensation deferred until a time when the house had less other pressing public business. The letter is printed in the *Pennsylvania Packet,* January 24, 1785. So far as I know, it has never been reprinted.

[2] Wilson, Janet, The Bank of North America. . . , *Penna. Mag. of Hist. and Biog.* **66**: 3–28, 1942.

[3] Foner, Philip S., ed., *The complete writings of Thomas Paine* II: 389, N.Y., Citadel Press, 1945.

sided against Robert Morris and the other financial leaders of Philadelphia, particularly in the Silas Deane-Arthur Lee controversy. Some of Paine's contemporaries accused him of accepting payment from Morris, and even modern scholars have been unable to explain why he waited until 1786 before declaring himself. The letter reprinted below shows that Paine affiliated himself with the bank early in the controversy—even before the charter was actually repealed. The letter is addressed "To the Printers of the Pennsylvania Gazette":

Please to insert the following in your next paper, and you will oblige yours &c.

THOMAS PAINE

Several publications having at different times appeared in the news-papers and in pamphlets, respecting the proceedings of the late House of Assembly against the *Charter of the Bank*, some of which I have been supposed to be the author of; I wish to have it known that I have never published any thing on that subject. As an individual, exercising my private opinion, I was concerned at the proceedings of the House on that business, because those proceedings appeared to me a breach of public faith, and hurtful to the general interest—and I constantly expressed this opinion, but published nothing on the subject.

The only sentiments of mine on this head (in writing) are in a letter to the Honorable Thomas Fitzsimmons, Esq; so long ago as the 19th of April; and as several gentlemen have seen that letter, and remember it in part, I think it best to give the whole, which is as follows:

Fitzsimmons, to whom this letter is addressed, was an Irish-born officer of the Revolution. He served as member of the Congress established under the Articles of Confederation in 1782, as a member of the Convention that formed the Constitution of 1787, and as a member of the national House of Representatives from 1789 until 1795.[4] He was one of the founders of the Bank of North America and became one of its trustees. Although a financier, he supported at least one popular cause, the campaign to have the government settle all its obligations to officers and men of the revolutionary army before demobilization It is possible that he was the author of an open letter to Paine (addressed as Common Sense) in the *Pennsylvania Packet*, December 7, 1782, asking him to write on the plight of army officers soon to be discharged without prospect of future pay or pension. In March, 1785, Fitzsimmons was one of twelve citizens who petitioned the Assembly to refrain from issuing paper money.[5] Paine wrote to him also on November 19, 1786, about various financial matters, including the bank,[6] and in March, 1787, Fitzsimmons served with Morris and others on a committee to consider Paine's bridge.[7]

Paine's letter in the *Pennsylvania Gazette* is dated April 19, 1785, and written from Brunswick.

Sir,

I received your favor of the 16th inst. by the stage-boat to Bordentown, which place I left yesterday for New York, but the bad weather being likely to detain me to-day, I shall dispose of it for the purposes of answering yours, and some other letters.

On the subject of the Bank, and the attack made on it by the Assembly, my sentiments and declaration have been free and open. It appears to me an ill-digested, precipitate, impolitic, faithless, piece of business, in which party and prejudice is put for patriotism. I observe, by a remark in your letter, that the matter struck you as it did me; for if the people of Pennsylvania cannot exercise their judgment as men, and their privileges as citizens, without being threatened with the power and made to suffer under the lash of government, freedom is a mere name. I am the more confirmed in the rectitude of my opinion on this case, because it operates with me, not in conjunction, but in opposition to my interest, and to those for whom I had some degree of predilection. The House is composed of men, with whom I have lived with more intimacy than with the generality of the citizens of Pennsylvania, and who have shewn more disposition to promote my interest than others have. The case between me and the Assembly stands thus:—They have advanced me five hundred pounds, and referred the matter to Congress, with an assurance of complying in such further measures as Congress shall adopt or recommend. But the House appears to me so exceedingly wrong in this business, both as to the matter and manner of it, that my private judgment on the case cannot go with them, and must go against them, disregarding consequences to myself.

As from a point of delicacy I absented myself from the company of the members while the matter respecting myself was depending. I had little opportunity of knowing what was going on in the House, and none of what was intended to be brought on. When the affair of the Bank broke out, I met several of the members (accidentally) and they expressed themselves on the subject with a sort of triumph. It is a great deal of thinking a man may sometimes do in a little time; and tho' I am not hasty in altering an opinion, yet in this case it was otherwise. It immediately struck me in a very different light to what it

[4] *Dictionary of American Biography.*

[5] Wilson, *op. cit.*, 6.

[6] Foner, ed., *Writings* II: 1259.

[7] *Ibid.* II: 1261.

did then, and appeared to me to extend to consequences they had not attended to. I mentioned to them what those consequences were, considering the matter in a *particular* view: That this quick rotation of doing and undoing, this facility of making and repealing laws, of granting charters and violating them, would eventually operate against themselves; because it had a tendency to strike at the constitution, which enabled the House, with so much ease, and often with too little deliberation, to exercise a power, that in the change and fluctuations of party might be dangerous to the rights and property of every man. That in a government where nothing was certain, the disposition to obedience would be so too—that acts, when so easily and frequently changed, lost the force and dignity of laws, and ceased to command respect—that their proceedings respecting the Bank was a dangerous precedent—that it came under the description of *governing too much*—and that, however gratifying it might be to their prejudice, it would, in my opinion, be fatal to their power.

It is not an agreeable thing for a man to stand in opposition to his friends, especially circumstanced with them as I was, for the matter respecting myself was then depending; but in this case I used the sincere freedom of a friend to them.

I found they put too much stress upon what they called the *dangerous influence of the Bank.* I replied, that the influence did not appear from its effects on any of the elections, and that the temper and complection of the House was an evidence that invalidated the assertion; to which I added, that I was apprehensive they had started a word that would change sides, and retort on themselves, for that it was probable a very general idea would arise, that the influence of the House was assuming over the privileges of citizenship would be thought more dangerous than the influence of the Bank, which, admitting it to operate, could extend only to the city, and in the late election not even to that.

In conversation with a principal Member of the House on the subject, I mentioned to him that the House ought to hear the Bank previous to their publishing the bill for public consideration, which is after the second reading; for that as *considering* naturally includes the idea of *judging,* the public ought to have the whole matter before them, and hear the Bank as well as its opposers; to which I added another reason, which was the reputation of the House; for that as the causes and reasons for a law are to be set forth in the preamble, if the causes and reasons assigned in the preamble of the bill for taking away the Charter of the Bank should, when the House came afterwards to hear the Bank, be proved to be ill founded and groundless, it would put the House in a disagreeable situation. I was surprised when he told me that the bill had been read the second time that morning, and ordered to be published. From the manner in which the bill crept through the House on the second reading, I am apt to think its advocates are suspicious of more embarrassment than they at first conceived.

As to paper money, which makes a part of the politics of the house, and was one of the causes that led to the attack on the bank, it is a subject that will bear much investigation. There may be cases in which paper money may be generally serviceable; but it is an expedient, that should be used with the greatest caution, or we shall have all the evils of depreciation both of money and morals over again.

There is but one way in which paper money can be secure and retain its value, and that the present system of the house does not provide for. If every house is to strike as much as it pleases, and exercise its discretion unlimited, where is the evil to end? I think the house misconceives its authority upon this case. It derives none from the Constitution, for that is silent on the subject, and only enables the house to make laws, not money, and provides no other means of revenue, than by taxation. Neither does the house copy the practice from any custom in the English government, for were the government of that country, to create a revenue by paper, instead of raising it by taxes, it would be considered as an attempt at arbitrary power, erecting itself independent of the grants of the public. Neither is the present mode conformable to that practised before the war, for then there was a restraining power over the conduct of the house; as to the quantity to be struck, which prevented depreciation.

Perhaps it may be said, that while it is not a legal tender, in all cases, the evils may not be so extensive; but the house conceives it has this power; if so, there is no security for property, where such power can be exercised; and it is inconsistent with the nature and genius of the Constitution of Pennsylvania, to admit it: for the Constitution declares that the *acquiring, possessing and protecting property, are the certain natural inherent inalienable rights of the citizens,* and therefore, any law of the Assembly which makes that a legal tender in payments, which is not equal in value to the property received, is a violation of the principle on which the Constitution is founded.

In writing you this letter, I have omitted all those matters which are familiar to the subject; such as the usefulness of a Bank in a commercial country; the conveniency it may be to government in certain cases, and sudden emergencies, and the services of the present Bank in the late war, and touched chiefly on such parts as are connected with it in its political consequences, and which it is probable do not occur to the generality of people.

I sincerely wish the affair had not arose. I have hitherto confined myself to such national matters only, as were connected with the independence of the country, and the issue of the contest. These having happily succeeded, I was in hopes every thing else would have gone well, and that the experience and discretion of the country and the government would have been

equal to its domestic concerns in all cases. I am much concerned to see it, otherwise, and sorry to find they want so much putting to rights.

In the progress of this affair, on the part of the Bank, one thing is necessary to attend to, which is, that of not relying too much upon it as a law case; the House in their preamble have set out with a declaration that the Bank is injurious to the state, and in its consequences dangerous; unless this idea be removed as publicly as it is asserted, the merely considering the question of taking away the Charter as a law question, will not be effectual to all the circumstances to which the subject extends.—But it is time to close my letter,

I am, Your obedient humble servant,
Thomas Paine

Eight other letters by Paine on the subject of the bank appeared in the *Pennsylvania Gazette* from March 29, 1786, to March 7, 1787. The first five of these appeared originally in the *Pennsylvania Packet*, and they are reprinted by Foner in his edition of Paine's works. Foner also reprints the eighth letter, which appeared in the *Pennsylvania Gazette*, March 7, 1787, but for some reason he does not mention the sixth and seventh. These appeared in the *Pennsylvania Gazette* on September 20, and November 8, 1786.

The first of these neglected letters reemphasizes the political foundation of Paine's support of the bank. The Assembly's act in revoking the charter, he regards as proof that a single legislature ruled by party prejudice "is capable of being made a compleat aristocracy for the time it exists." Because of "the superabundance of its power, and the uncontrouled rapidity of its execution," it may become "as dangerous to the principles of liberty as that of a despotic monarchy." This is an expansion of the denunciation in his letter to Fitzsimmons of the arbitrary conduct of the Assembly in reversing the act of a previous session. Most of the letter is devoted to the disadvantages of a unicameral legislature and is not specifically on financial matters.

The second letter, on paper money, is entirely economic, however, and demands fuller discussion. As we have seen, the issuance of paper money was closely involved with the revoking of the charter of the bank. In other states besides Pennsylvania the demand for paper money raged. Paine, therefore, described paper money in November, 1786, as "both the bubble and the iniquity of the day." Some bad people have been concerned in the paper money movement, he asserts, but the far greater part are innocent but misled. These men are so confused that they maintain such absurdities about paper money as that it has improved the country and carried on the war. Paine explains that credit, not paper money, did these things and that the credit which once existed has failed by non-performance, by the country being involved in debt, and by the levity and instability of government measures.

Even the name "paper money," Paine asserts, reflects confusion and error. The name should not be "paper money," but instead "bills of credit." The issuance of these bills amounts to "anticipating or forestalling the revenue of future years and throwing the burden of redemption on future assemblies." It is like a man mortgaging his estate and leaving his successors to pay it off. But even this is not the worst of paper money. It banishes hard money and diminishes the value of revenue. The State of Pennsylvania cannot now emit bills of credit because the assembly which makes such an emission cannot bind future assemblies. The precedent of revoking the charter of the bank has become a precedent for any assembly to undo what another has done. It keeps an assembly from doing anything extending beyond the year in which it sits. People will no longer have confidence in the acts of any assembly which requires the sanction of another assembly to carry them out. "The politicians of the project for revoking the bank charter (and it was besides most wantonly done) to use a trite saying, aimed at the pidgeon and shot the crow—they fired at the bank and hit their own paper."

We have no legal tender bills in Pennsylvania, Paine continues, but since they exist in other states, he presents a commentary on them. He begins with the general maxim that "the abuse of any power always operates to call the right of that power in question." Consequently, to judge of the right or power to make legal tender acts requires a study of principles of civil government. The fundamental principles of civil government are security of our rights and persons as freemen and security of property. Legal tender acts cannot be justified under civil government since they take away a man's share of civil and natural freedom and render his property insecure. It would be a strange law which would compel a man with six hundred dollars of silver money to deliver them up to any person who discovered he had them, in exchange for one hundred dollars of paper money. This result of a legal tender act would violate the principles of civil government.

Principles of civil government, furthermore,

"compel the exact performance of engagements entered into between man and man." A man cannot undertake to deliver a quantity of wheat and deliver rye instead; likewise a man cannot contract to pay in hard money and actually pay in paper or anything else. Civil government also "disowns the practice of retrospective laws." A legislature, for example, cannot punish a man for a crime committed before the law against it is made. The principle against retroactive laws extends to property as well as life. A law made after a contract is made cannot govern that contract any more than a law to punish crime can be applied to acts prior to the law. All legal tender acts which apply to the alteration of past contracts, Paine asserts, are contrary to principles of civil government.

But even if there were no illegality in tender laws, Paine concludes, they are defective on other considerations. "They cannot bind all and every interest in the state, because they cannot bind the state itself. They are, therefore, compulsive where they ought to be free: that is, between man and man, and are naturall free where, if at all, they ought to be compulsive: for in all cases where the state reserves to itself the right of freeing itself, it cannot bind the individual, because the right of the one stands on as good ground as that of the other." [8]

This essay was written a year and a half after the private letter to Fitzsimmons. Like the other subsequent letters on the bank, it was written on a specific issue in the controversy. For a general discussion of Paine's position on the bank, however, I shall revert to the broad principles of the letter to Fitzsimmons.

In Paine's day as well as ours, a banking institution was popularly considered as a vested interest, and the opponents to it considered themselves as champions of the rights of the people against the power of privilege. Paine's letter to Fitzsimmons consists of a paradox, as amazing to us as it probably was to Paine's contemporaries, that the bank actually stood for popular rights and liberties, whereas those who attacked it in the Assembly represented arbitrary power and the denial of natural rights. Equally paradoxical is his view that to rescind the charter of the bank would be a kind of "governing too much." Actually Paine advocates government by principle rather than government by whim, an ideal to which both conservatives and radicals subscribe, but his application of political principles to the particular situation was undoubtedly paradoxical, although not inconsistent with his other writings. What is most important in the letter is the evidence that Paine regarded the bank as a democratic institution, and its opponents as enemies to freedom. Of course it would have been politic at that time, as it is now, for both advocates and opponents of any measure to dedicate themselves in public to freedom and democracy, but there is no evidence of any kind that Paine was insincere. The fact, which he stresses in his letter, that his personal advantage lay with the assembly rather than with the bank

[8] Paine's denunciation of legal tender acts suggests that he may have been the author of the following letter in the *Pennsylvania Packet*, February 8, 1780. The letter is signed "T. P." and addressed:

"To the Honorable the Congress of the United States.

The Law which makes the Continental Money, a legal tender for old debts, was made in consequence of a recommendation of Congress for that purpose. This law, though originally well intended, has done more mischief in our country than the British army. It has been fatal to thousands of the most worthy and helpless members of the community. It has depreciated the money more than all the counterfeit Sir William Howe ever lent among us; and lastly, it has contributed more to extirpate religion and virtue from our country in the space of four years, than the writings of Hume and Bolingbroke could have done in a century. As the above law originated in Congress, the public have a right to look to the Congress for a resolution recommending a repeal of it. Several of the Legislatures of these States, are looking up to you, gentlemen, for a resolution for that purpose. The perishing widow and orphan, and the aged citizen, whose groundrents and leases once afforded him a handsome subsistence, have more than once beset your doors with their cries. 'Take care (to use your own emphatical words) that they rise no higher.'—Say not the mischief is done. There are hundreds and thousands of people in our country, whom a repeal of the *tender law* would immediately restore to competence and happiness. Let no *false* reasonings upon the subject of money lead you to delay this necessary resolution. That policy is ever weak, which is maintained at the expense of justice, and, to use the words of a member of that illustrious and immortal body of men who composed the Congress of 1774, whilst America was innocent, and knew nothing of the cruel despoiler of her virtue (for that name best befits the law for making the Continental Money a legal tender for old debts,) 'NOTHING (said he in a debate where the subtleties of policy were argued against the plain rules of equity) THAT IS MORALLY WRONG, CAN EVER BE POLITICALLY RIGHT'."

The references to widows, orphans and the aged anticipate a passage in Paine's *Dissertations on Government* (see below). If the T. P. of this letter is really Thomas Paine, the reference to the writings of Hume and Bolingbroke shows that he was not quite as original in *The Age of Reason* as he pretends to be in its autobiographical passages. For this reason, the letter cannot be assigned to Paine with certainty.

shows that he is not substituting "party and prejudice . . . for patriotism."

Paine's chief argument is that a legislature is a continuous, responsible body, even though its members may change, and that the members of one session are bound by the actions of previous sessions. To Paine, the opposition to the bank looked like government by personal prejudice and hence "governing too much." He saw a danger to the "rights and property of every man" in "this quick rotation of doing and undoing, this facility of making and repealing laws, of granting charters and violating them." His demand for legislative stability resembles on the surface Edmund Burke's famous principle of prescription, which Paine was to denounce several years later. Burke argued that personal privileges once granted by custom may never be taken away, and Paine seems to believe that institutional privileges once granted by a legislature must not be abrogated by succeeding legislatures. In his *Dissertations on Government* Paine clarifies his position, however, and tells us which measures must be respected by succeeding legislatures and places limits on the duration of immunity from change. He makes a distinction between laws and matters of negotiation. The latter comprehend all kinds of public business which the assembly transacts with individuals or particular groups for special needs of the state. These acts, such as borrowing money or purchasing goods, are contracts, and like all contracts are binding for the duration of the agreement. "No law made afterwards can apply to the case, either directly, or by construction or implication: for such a law would be a retrospective law, or a law made after the fact." [9] A law proper, however, unlike these contracts, binds every individual of the commonwealth. Since laws cover every citizen, "they may be altered, amended and repealed, or others substituted in their places, as experience shall direct, for the better effecting the purpose for which they were intended." If Paine went no further than this with his system, it would indeed deserve to be called conservative. But even contracts, he believes, cannot last forever. "As we are not to live forever ourselves, and other generations are to follow us, we have neither the power nor the right to govern them, or to say how they shall govern themselves." [10] This sentence is so contrary to conservatism that it could easily be fitted into *The Rights of Man*, Paine's denunciation of Burke's theory of prescription. Paine adds that since thirty years is the average duration of a generation, thirty years should also be the terminal point for all laws and acts. "Such as were proper to be continued, would be enacted again, and those which were not, would go into oblivion." [11] The granting of a charter to the bank, Paine regarded as being in the first category, a contract between the state and a particular group of individuals, and hence an agreement that could not be lightly set aside. He did not specifically apply his principle of thirty years duration to the charter of the bank, but strongly suggested that the application should be made. He candidly admitted that a weakness in the charter was its failure to limit its duration.

The other arguments in Paine's letter to Fitzsimmons are also presented at greater length in his *Dissertations*. He further animadverts on the haste and secrecy by which the bill to repeal was twice read to the House in one day. He charges that the committee of inquiry which drew up the bill made no inquiries except concerning their own opinions, that they were "a committee of inquiry making no inquiry at all, and giving an opinion on a case without inquiring into the merits of it." [12] He repeats in almost identical words his argument that the assembly derives no authority from the Constitution to make paper money,[13] and asserts that "the assumed authority of any assembly in making paper money, or paper of any kind, a legal tender, or in other language, a compulsive payment . . . is a most presumptuous attempt at arbitrary power." [14] After further consideration of the evils of paper money, he concludes with the contention that "all tender laws are tyrannical and unjust, and calculated to support fraud and oppression." [15]

The political implications of these arguments are less apparent in the *Dissertations* than in the letter to Fitzsimmons. In a private letter to a friend skilled in political theory, Paine could emphasize the points of law involved. For popular consumption, however, he realized that such abstract considerations would not do. The *Dissertations*, consequently, stress the concrete advantages to the state offered by the bank and refute the contention that it is in any way dangerous—arguments likely to bear most weight with "the generality of people."

[9] Foner, ed., *Writings* II: 376–377.
[10] *Ibid.* II: 395.
[11] *Ibid.* II: 395.
[12] *Ibid.* II: 389.
[13] *Ibid.* II: 406.
[14] *Ibid.* II: 407.
[15] *Ibid.* II: 409.

Paine did not foresee a bank as an undesirable "accumulation of wealth," giving profit to its stockholders or influencing the economic life of communities for the benefit of individuals. He saw nothing wrong in "accumulation of wealth" in itself; nor for that matter do the most radical of economic thinkers today. What the radical of today opposes is the accumulation of private wealth and monopoly. Paine probably shared this view, for he supported the bank specifically because he regarded it as an agency to prevent private monopoly. If a farmer comes into a city with produce, he argued, only a few merchants could purchase it with ready money and these few would command nearly the whole market, "but, by means of the bank, this monopoly is prevented, and the chance of the market enlarged." [16] Monied men, he observed in a later letter, view a public bank as "standing in the way of their private interest." [17] Neither Paine's political theory for statesmen nor his specific arguments for the generality of men are fundamentally conservative. He consistently defends property rights by human rights and nowhere suggests that he would sacrifice the latter for the former. Although he characterizes money as having "something in it sacred that is not to be sported with, or trusted to the airy bubble of paper currency," money is sacred only when "considered as the fruit of many years' industry, as the reward of labor, sweat and toil, as the widow's dowry and children's portion, and as the means of procuring the necessaries and alleviating the afflictions of life, and making old age a scene of rest." [18] Those who, on the other hand, benefit by paper currency and repeal of the bank's charter are unscrupulous traders who buy up great holdings on credit, agitate for increased issues of paper money, and pay their debts with depreciated currency which they get at little price." [19]

Today we should interpret Paine's support of the bank in the light of these specific arguments used in its favor, not in the light of hasty contemporary accusations by enemies of the cause he was championing, nor in the light of a presupposition from a later day that supporting any financial institution is in itself a stamp of conservatism.

[16] *Ibid.* II: 398.
[17] *Ibid.* II: 424.
[18] *Ibid.* II:405.
[19] *Ibid.* II: 406.

COUNT RUMFORD AND THE CALORIC THEORY OF HEAT[1]

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THE name of Count Rumford is found in almost every textbook on general physics. Generally, however, one finds mention only of his cannon-boring experiments and a paraphrase of Professor John Tyndall's comment (1),[2] "Rumford, in this memoir annihilates the material theory of heat. Nothing more powerful on the subject has since been written."

The serious student is puzzled by the statement that a single experiment could bring about the complete downfall of the well established caloric theory, or that one set of results should credit Count Rumford with having led the first successful attack on the accepted theory of heat of his day. The answer is, of course, that Rumford's great contemporary reputation was by no means based solely on this experiment with heat from friction, but on a long series of experiments extending over a period of thirty years, which attacked the caloric theory from many different points of view.

Rumford originated neither the energy theory of heat nor some of the experiments which he carried out to prove this theory. The work of Boyle, Hooke, Leibnitz, and Locke seems all to have had its influence on Rumford's achievement. Rumford's most famous experiment is similar in principle to one reported by Boyle, and he practically quotes Hooke in some of his descriptions of heat as a form of energy. Nevertheless, his outstanding experimental skill, his endless search for ways to attack the caloric theory, as well as his international fame as a soldier, statesman, and philosopher, made him one of the first successful advocates of our modern concept of heat.

At the end of the eighteenth century the two opposing theories of the nature of heat were being vigorously debated. The caloric theory explained the nature of heat as a fluid, and the energy theory considered it "a mode of motion." To appreciate Count Rumford's preeminence as an advocate of the energy theory, one must study the contemporary caloric theory in detail and consider what experimental tests led Rumford to disbelieve the existence of heat as a material fluid.

Caloric was considered to be a fluid capable of penetrating all space and able to flow in and out of all substances. Caloric was self-repulsive and was strongly attracted by matter. Matter was believed to be made up of atoms held together by their mutual gravitational attraction. Each atom was surrounded by an atmosphere of caloric, whose density diminished rapidly as one receded from the center of the atom. The height of the caloric atmosphere was a function of temperature. Most of the known phenomena of heat seemed consistent with this material theory. A caloric explanation and the work Rumford did to refute the caloric arguments are the subjects of this paper.

THERMAL EXPANSION

The caloric theory supplied an obvious answer to the problem of thermal expansion and contraction. Heating a body consisted merely of adding the fluid caloric to the body. Since the fluid occupied space, one would expect an expansion. On cooling, the caloric fluid was removed and hence the body contracted. The difference between solids, liquids, and gases was considered to lie essentially in the degree of gravitational attraction between the atoms of the substance. With small amounts of heat, the caloric repulsion was not strong and the atoms were tightly bound by strong gravitational attraction. As the temperature of a body was increased, the attraction became less as the thermal repulsion became greater. In a liquid, the caloric content was sufficiently high so that the atoms were not held in a rigid position by the mutual gravitational attraction. In a gas, the gravitational attraction was considered inoperative. This theory, therefore, led one to expect the observed effect that the expansion of a gas was greater than in the case of a liquid, which in turn was greater than that of a solid.

Concerning the problem of thermal expansion, Count Rumford wrote as follows (2):

> It seems probable that motion is an essential quality of matter, and that rest is nowhere to be found in the universe. . . . There are many appearances which seem to indicate that the constituent particles of all

[1] Supported in part by a grant from the Penrose Fund of the American Philosophical Society.

[2] Numbers in parentheses indicate references at end of article.

bodies are also impressed with continual motions among themselves, and that it is these motions (which are capable of augmentation and diminution) that constitute the heat or temperature of sensible bodies.

On a superficial view of the subject, it may perhaps appear difficult to reconcile solidity, hardness, and elasticity with those never-ceasing motions which we have supposed to exist among the constituent particles of all bodies. . . . No reasonable objection against this hypothesis (of the incessant motions of the constituent particles of all bodies), founded on a supposition that there is not room sufficient for these motions, can be advanced; for we have abundant reason to conclude that if there be in fact any indivisible solid particles of matter, these particles must be so extremely small, compared to the spaces they occupy, that there must be ample room for all kinds of motion among them.

If the motions of its constituent particles were diminished . . . it seems reasonable to conclude that their elongations would become less, and consequently that the volume of the body would be contracted, but if the motions of these particles were increased, we might conclude, *a priori*, that the volume of the body would be expanded.

According to the caloric theory, when a substance was cooled and the caloric fluid extracted from it, it contracted and hence became denser. It is, therefore, not surprising to find Rumford studying carefully the case of water near its freezing point, where its change in density is anomalous. In his "Account of Some New Experiments on the Temperature of Water at its Maximum Density," he showed that water at 41° F. is denser than water at 32° F. by demonstrating that water at this higher temperature sinks in water at the lower temperature.

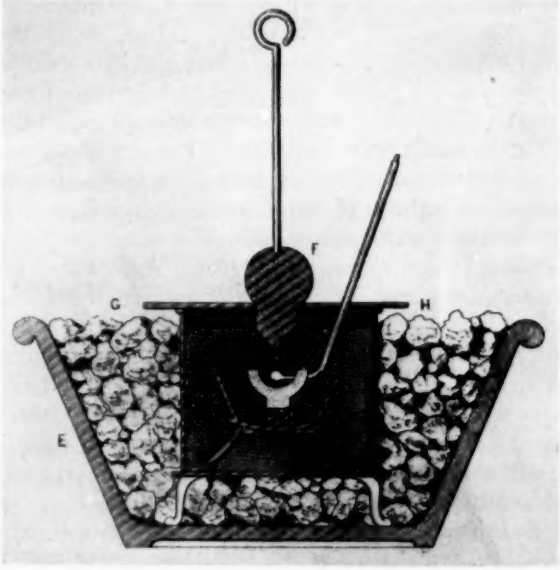

FIG. 1. Apparatus used to show that water at 41° F. sank in water at 32° F.

The apparatus which the Count used in this series of experiments is shown in figure 1. A vessel *H* filled with water was brought to equilibrium at 32° F. by surrounding it with ice. A tin ball *F* with a conically pointed end was cooled to a temperature of 42° F. and mounted as shown over the thermometer *D* mounted in a cork cup *C*. Rumford's explanation of his results was as follows (3):

> I knew that the particles of ice-cold water which were thus brought into contact with the conical point would not fail to acquire some small degree of heat from the relatively warm metal, and I concluded that if the particles of water so warmed should in fact be *heavier* than they were before, in consequence of this small increase in temperature, they must necessarily *descend* in the surrounding lighter ice-cold liquid, and as the heated metallic point was placed directly over the cork cup, and fixed immovably in that situation, I foresaw that the descending current of warm water must necessarily fall into that cup and at length fill it, and that the presence of this warm water in the cup would be announced by the rising of the thermometer.
>
> The result of this very interesting experiment was just what I expected: the conical metallic points had not been in contact with the ice-cold water more than 20 seconds when the mercury in the thermometer began to rise, and in 3 minutes it had risen three degrees and a half.

In other parts of this paper, and also in a paper entitled, "An Account of a Curious Phenomenon Observed on the Glaciers of Chamouny," Count Rumford arrived at the temperature of 41° F. as that for the maximum density of water. His primary purpose in carrying out these experiments, however, is expressed in the conclusion of the paper quoted above:

> These experiments ought not to be regarded as suitable for determining with great exactness the temperature at which the density of water is at a maximum, but rather as proving that this temperature is really several degrees of the thermometric scale above that of melting ice.

Count Rumford offered no explanation of this phenomenon using the energy theory of heat. However, he apparently felt that the fact that the removal of the fluid caloric did not cause the water continuously to contract was a serious objection to the material theory.

CONDUCTION

According to the caloric theory, conduction of heat took place because of the great attraction between matter and caloric. The less caloric a body had, the greater was the attraction of its atoms for the caloric fluid. Thus, when heat was added to one end of a solid bar, the atoms at the heated end acquired more caloric than their neighbors, and having more, their attraction for caloric became less. The neighboring atoms attracted excess caloric away and continued to do so until all the atoms of the substance had achieved the same caloric atmospheres, that is, until temperature equilibrium was set up.

Count Rumford carried out a long series of investigations on the "Propagation of Heat in Various Substances" (4). He studied the difference in the conducting power of many substances, in different forms and conditions. He showed that his observations warranted the conclusion that in a single substance, the conducting power increased with the compactness of the structure as would be predicted by the caloric theory. Carrying the caloric theory of conduction to its logical conclusion, conduction through a vacuum should not occur since no attractive material particles were present. Count Rumford was the first to prove that this prediction of the caloric theory was false and that heat was conducted through a vacuum. In his "New Experiments upon Heat," he describes his apparatus as follows (5):

> In the middle of a glass body, of a pear-like form . . . I suspended a small mercurial thermometer . . . by a fine thread of silk, in such a manner that neither the bulb of the thermometer, nor its tube, touched the containing glass body in any part. . . . There was an opening at the bottom of the glass body, through which the thermometer was introduced; and a barometrical tube being soldered to the opening, the inside of the glass body was voided of air by means of mercury, and this opening being afterwards sealed hermetically, and the barometrical tube being taken away, the thermometer was left suspended in a Torricellian vacuum. . . .
>
> This instrument being fitted to a small stand . . . in such a manner that the glass body remained in a perpendicular situation, I placed it in my room, by the side of another enclosed thermometer which was surrounded by air, and observed the effects produced on it, by the variation of Heat in the atmosphere. I soon discovered, by the motion of the mercury in the inclosed thermometer, that the Heat passed through the Torricellian vacuum; but it appeared plainly, from the sluggishness or great insensibility of the thermometer, that the Heat passed with much greater difficulty in this medium than in common air.

The caloricist's answer to such experiments was the introduction of radiant caloric which was transmitted not by the attraction of material for the caloric, but in the absence of material, by the self-repulsive property of the caloric fluid.

Rumford performed many and diverse experiments to prove the passage of heat through a vacuum, and concluded (6):

> The results of the experiments performed with these instruments differed little or not at all from those made with the apparatus previously described, therefore the fact of the transmission of heat through the Torricellian vacuum was established beyond any doubt.
>
> These results are sufficiently known to the learned world; now the question arises as to how these results can be reconciled with the theory which at the present day has been adopted in regard to *caloric*. I must confess freely, that, however much I might desire it, I never could reconcile myself to it, because I cannot by any means imagine how heat can be communicated in two ways entirely different from each other.

RADIATION AND REFLECTION

The radiation of heat was known to be more influenced by the nature and condition of the surface of a hot body than by the material of the body. The differences in the types of surfaces for holding caloric were explained simply. A surface which was polished smooth was considered to have all the molecules lying in a plane, and all the molecules as close together as possible. Thus, since the greatest amount of mass was found at this kind of a surface, the caloric was most tightly bound and could be lost only with the greatest of difficulty. This defined a poor radiator. That it was a good reflector was easily predicted: since there already existed a tightly bound caloric surface layer, it would repel any further caloric coming up to the surface. Good radiators are rough surfaces. This meant that for any given area of surface, there was a small amount of mass lying in a plane, and hence the caloric was held only by a few groups of atoms at any one place. The neighboring atoms did not have the cumulative effect present when there were more of them as neighbors, so that the caloric might escape more easily, and one obtained a good radiator.

Rumford believed that this caloric picture provided no explanation of the radiation of cold, which he considered to be different in kind from that of heat, but obeying the same laws. In one of his

assaults on the caloric theory, he describes as follows (2) an experiment based on his "frigorific radiation."

Let the two opposite ends of the cylinders *A* and *B* [fig. 2] represent the two vertical metallic disks of equal dimensions, which were presented at the same time to the ball of the thermoscope [air thermometer] *C*. . . .

The disk *A* being at the temperature of 32° F., and the disk *B* at 112° F., while the ball of the thermoscope *C* and all other surrounding bodies were at 72°, it was found that the temperature of the thermoscope was not changed by the simultaneous action of these two bodies, the one hot and the other cold.

Now, as the temperature of the ball of the thermoscope is an arithmetical mean between that of the disk *A* and that of the disk *B*, it is reasonable to suppose that the thermoscope receives just as much more caloric from *B* than it gives to it as it gives to *A* more than it receives from it; and if that be the case in fact, it is evident that the simultaneous actions of the two disks on the ball of the thermoscope (or the traffic which they carry on with it in caloric) can neither tend to increase nor to diminish the original stock of that substance belonging to the instrument; consequently the instrument will neither be heated nor cooled by these interchanges, but will continue invariably at the same constant temperature.

When the surface of the metallic disk *B* was blackened by holding it over the flame of a candle, the intensity of its radiation at the given temperature (that of 112°) was found to be very considerably increased; and when (being so blackened) it was again presented to the ball of the thermoscope at the same distance as in the last-mentioned experiment, and the cold disk *A* (at the temperature of 32°) was placed opposite to it at an equal distance (as represented in fig. [2]) the thermoscope, instead of continuing to retain its original temperature (that of 72°), was now gradually heated.

There is nothing, it is true, in that event, which appears difficult to explain on the assumed (caloric) principles, for, if the quantity of radiant caloric emitted by the disk *B* be increased by blackening its surface, the quantity received from it by the ball of the thermoscope must be increased also, and that additional quantity must, of course, tend to raise the temperature of the instrument. But here is an experiment which cannot be explained on those principles.

The surface of the cold disk *A* having been blackened as well as that of the hot disk *B*, when both disks (blackened) were again presented at equal distances to the ball of the thermoscope (as represented in fig. [2]) it was found that the original temperature of the thermoscope remained unchanged.

The result of this most interesting experiment proves that the ball of the thermoscope was just as much cooled by the influence of the cold blackened disk as it was heated by the hot blackened disk.

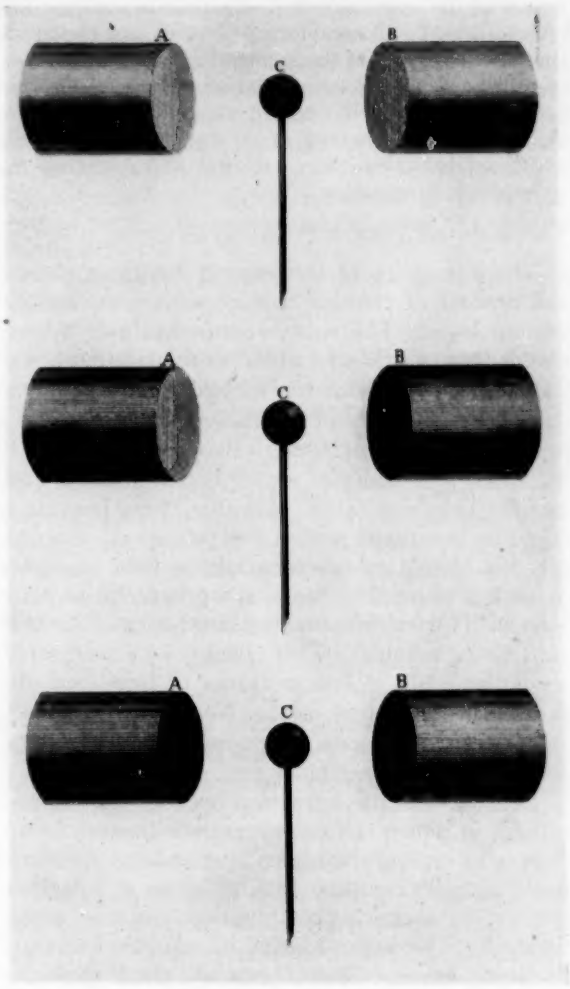

FIG. 2. Apparatus used to study radiation from different kinds of surfaces.

Now, as it was found by experiment that the intensity of the radiation of the disk *B* was *increased* by the blackening of the surface of that disk, we must conclude that the intensity of the radiation of the disk *A* was likewise *increased* by the use of the same means, but if those radiations be caloric, emitted by those bodies . . . how did it happen that the ball of the thermoscope, instead of being *more heated* by the additional quantity of caloric which it received in consequence of the blackening of the disk *A*, was actually *more cooled?*

It may perhaps be said by the advocates for the hypothesis in question, that the blackening of the surface of the disk *A* caused a greater quantity of caloric to be sent off to it by the balls of the thermoscope. Without insisting on an explanation of the mode of

action of the cause which is supposed to produce this effect . . . , I will content myself with just observing that as the surface of the opposite disk was also blackened, this supposed augmentation of the quantity of caloric, emitted by the ball of the thermoscope, occasioned by the blackening of the surface of the bodies presented to it, can be of no use in explaining the phenomena in question.

CHANGE OF STATE

The advocates of the caloric theory explained the process of change in state with what seemed careful logic. The rise in temperature of a body meant that caloric was added to the substance, expanding it because of the mutual repulsion of the caloric atmospheres of the individual atoms. The addition of the heat which raised the temperature increased the caloric atmospheres around each atom. This was called "sensible" heat because it could be measured with a thermometer. Eventually the atom's caloric atmospheres had increased to such a point that the atoms proceeded to combine with the caloric, turning into essentially a new substance, a liquid. This change was necessarily accompanied by a disappearance of heat from the sensible to the latent form. No heat was considered lost in the process, since on cooling the caloric reverted to the sensible form.

Thus besides the attraction between matter and caloric, in which the caloric merely formed an atmosphere around the atoms and molecules, caloric could actually combine with an atom in a fashion similar to chemical combinations of the atoms themselves, the caloric losing its sensible form and becoming latent. The "chemical" combination of an atom with caloric produced a new compound in which neither the original atom nor the caloric retained its identity. It was believed to be completely analogous to ordinary chemical reaction. It took place, as do ordinary chemical reactions, only in definite proportions and under definite circumstances.

The case of vaporization seemed an additional check on the validity of the caloric theory. The requirement for a reaction to take place between the sensible caloric and an atom was a critical amount of caloric around each atom. The compression of an iron bar by hammering apparently squeezed out sensible caloric, or a cylinder of gas which was compressed emitted caloric and became hotter. It seemed obvious that sensible caloric could be squeezed from a body by artificially pushing the atoms together into a closer proximity than the mutual repulsion of their caloric atmospheres would allow. Thus if one put pressure on a substance near its boiling point, some of the sensible caloric would be lost from the substance and a higher temperature would be necessary before sufficient caloric would be available to the atom for the vaporization reaction to occur. This fact was amply borne out by experiment.

Count Rumford's explanation of the phenomenon of change of state was expressed thus (2):

We have not sufficient data to enable us to form a distinct idea of the nature of the change which takes place when a solid body is melted; but as fusion is occasioned by heat, that is to say, by an augmentation of that action which occasions expansion, if expansion be occasioned by an increase of the motions of the constituent particles of the body, it is, no doubt, a certain additional increase of those motions which causes the form of the body to be changed, and from a solid to become a fluid substance.

As long as the constituent particles of a solid body which are at the surface of that body do not, in their motion, pass by each other, the body must necessarily retain its form or shape, however rapid those motions or vibrations may be; but as soon as the motion of these particles is so augmented that they can no longer be restrained or retained within these limits, the regular distribution of the particles which they acquired in crystallization is gradually destroyed, and the particles so detached from the solid mass form new and independent systems, and become a liquid substance.

Whatever may be the figures of the orbits which the particles of a liquid describe, the mean distances of those particles from each other remain nearly the same as when they constituted a solid, as appears by the small change of specific gravity which takes place when a solid is melted and becomes a liquid; and . . . it is evident that the additional motion they must necessarily acquire, in order to their taking the liquid form, cannot be lost, but must continue to reside in the liquid, and must again make its appearance when the liquid changes its form and becomes a solid. It is well known that a certain quantity of heat is requisite to melt a solid, which quantity disappears or remains latent in the liquid produced in that process; and that the same quantity of heat reappears when this liquid is congealed and becomes a solid body.

KINETIC THEORY

The caloricists did not bring forth any convincing arguments to explain evaporation and sublimation, but one finds Rumford devoting many pages of his memoirs to discussing these changes of state. Although his language seems odd today, one can recognize in his discussion the elementary concepts of kinetic theory (7).

It seems highly probable that many appearances which have been attributed to very different causes

are in fact owing to *intense Heat* existing and producing the effects proper to it in situations where its existence has not even been suspected.

There is no reason to suppose that it is possible for ice to be reduced to steam without being previously melted; and it is well known that ice cannot be melted with a lower degree of *Heat* than that of 32° of Fahrenheit's scale: but in the midst of winter, in the coldest climates, and when the temperature of air of the atmosphere, as shown by the thermometer, has been much below 32°, ice, exposed to the air, has been found to evaporate.

How can we account for this event, except that it be by supposing that some of the particles of air which accidentally come into contact with the ice are so hot, as not only to melt the small particles of ice which they happen to touch, but also to reduce a part of the generated water to steam?

Mercury has been actually found to evaporate under the mean temperature of the atmosphere. What a striking proof is this . . . that very intense *Heat* may be generated, or exist where it would not naturally be expected to be found! And does not the evaporation of water under the mean temperature of the atmosphere afford another proof of this last fact?

We ought to be on our guard in forming an opinion with respect to the intensity of the Heat which may exist in the invisible insulated particles of matter of any kind that may be scattered about in a given space, or which may float in any fluid, where neither our feelings nor our thermometers can possibly be sensibly affected by it. A thermometer can do no more than indicate the mean of the different temperatures of all those bodies or particles of matter which happen to come into contact with it. If it be suspended in air, it will indicate the mean of the temperatures of those particles of air which happen to touch it; but it can never give us any information respecting the relative temperatures of those particles of air.

Count Rumford undertook careful investigations to demonstrate the existence of particle motion in a liquid at a uniform temperature. These he described in a paper entitled "Of the Slow Progress of the Spontaneous Mixture of Liquids" (8). His description of the experiment was, in part, as follows:

Having procured a cylindrical vessel of clear glass . . . provided with a scale divided from the bottom upwards into inches and lines [1/12 inch], I put it on a firm table in the middle of a cellar, where the temperature which seemed to be tolerably constant, was 64 degrees of Fahrenheit's scale.

I then poured into this vessel, with due precautions, a layer of a saturated aqueous solution of muriate of soda, 3 inches in thickness, and on to this a layer of the same thickness of distilled water. This operation was performed in such a way that the two liquids lay one upon the other without being mixed, and when everything was at rest I let a large drop of the essential oil of cloves fall into the vessel. This oil being specifically heavier than water, and lighter than the solution of muriate of soda on which the water rested, the drop descended through the layer of water; when, however, it reached the neighborhood of the surface of the saline solution it remained there, forming a little spherical ball, which maintained its position at rest, as though it were suspended, near the axis of the vessel.

After the little apparatus mentioned above had been left to itself for twenty-four hours, . . . I found that the little ball had risen 3 lines. The next day, at the same hour, . . . I found that it had risen about 3 lines more, and thus it continued to ascend about 3 lines a day for six days, when I put an end to the experiment.

These experiments showed that the mixture went on continually, but very slowly, between various aqueous solutions employed and the distilled water resting upon them. . . . Is [this mixture], perhaps, the result of a peculiar and continual motion common to all liquids, caused by the instability of the equilibrium existing among their molecules?

It is interesting to observe that when the modern kinetic theory was worked out, Einstein (9) pointed out that the spontaneous mixture of liquids of different densities affords one of the most direct experimental proofs of the predictions of this theory.

THE WEIGHT OF HEAT

The explanation of many caloric phenomena was based on the strong attraction between caloric and matter. Because of this attraction, one would expect that the force between the caloric in a body and the earth, namely the weight of caloric, should be a measurable quantity. One of Count Rumford's major investigations was an attempt to measure this weight of heat both in its "sensible" and "latent" form. This experiment is described in Rumford's own words as follows (10):

Having provided three bottles *A*, *B*, and *C*, as nearly alike as possible, . . . into the first, *A*, I put 4214.28 grams of water . . . ; into the second bottle, *B*, I put a like weight of spirit of wine . . . ; and, into the bottle, *C*, I put an equal weight of mercury.

These bottles, being all hermetically sealed, were placed in a large room . . . where the air appeared to be perfectly quiet, and, being suffered to remain in this situation more than twenty-four hours, the heat of the room (61°) being kept up all the time with as little variation as possible. . . . They were all weighed, and were brought into the most exact equilibrium with each other, by means of small pieces of very fine silver wire, attached to the necks of those of the bottles which were the lightest.

This being done, the bottles were all removed into a room in which the air was at 30°, where they were suffered to remain, perfectly at rest and undisturbed, forty-eight hours. . . . At the end of forty-eight hours . . . I found . . . the bottles *A* and *B* remaining in the most perfect equilibrium.

I now removed the bottle *B* from the balance, and put the bottle *C* in its place; and I found that that likewise remained of the same apparent weight as at the beginning of the experiment, being in the same perfect equilibrium with the bottle *A* as at first.

I afterwards removed the whole apparatus into a warm room, and causing the ice in bottle *A* to thaw, and suffering the three bottles to remain till they and their contents had acquired the exact temperature of the surrounding air, . . . comparing them together I found their weights remained unaltered.

It is very certain that the quantity of heat lost by the water must have been very considerably greater than that lost by the mercury, the specific quantities of heat in water and in mercury having been determined to be to each other as 1000 to 33; but this difference in the quantities of heat lost produced no sensible difference on the weights of the fluids in question.

Water, upon being frozen, has been found to lose a quantity of heat amounting to 140 degrees of Fahrenheit's thermometer; or . . . the heat which a given quantity of water, previously cooled to the temperature of freezing, actually loses upon being changed to ice, if it were to be imbibed and retained by an equal quantity of water, at the given temperature (that of freezing), would heat it 140 degrees. . . .

The capacity of water to receive and retain heat—or what has been called the specific quantity of heat—has been found to be that of gold as 20 to 1; consequently, the heat which any given quantity of water loses upon being frozen, were it to be communicated to an equal weight of gold at the temperature of freezing, the gold, instead of being heated 140 degrees, would be heated $140 \times 20 = 2800$ degrees, or, would be raised to a bright red heat.

It appears, therefore, to be clearly proved by my experiments, that a quantity of heat equal to that which 4214 grains (or about 9¾ oz.) of gold would require to heat it from the temperature of freezing water to be red hot, has no sensible effect upon a balance capable of indicating so small a variation of weight as that of 1/1,000,000 part of the body in question.

HEAT FROM MECHANICAL WORK

The caloric theory of heat visualized the caloric fluid as squeezed out of a body under the action of mechanical work. Considering the atoms of a gas as essentially unhindered by the gravitational attraction of neighboring gas atoms, they were held apart by the mutual repulsion, of their caloric atmospheres. By pushing the atoms closer together, one was merely overcoming the thermo-repulsion by mechanical force, and in so doing, squeezing the extra caloric out of the gas, which gave off heat. If the experiment was reversed, and the gas expanded, the temperature fell, since the atoms would be farther apart than their caloric atmospheres would keep them. This would result in a lack of caloric surrounding their atoms, making them colder. Experimental study of the removal

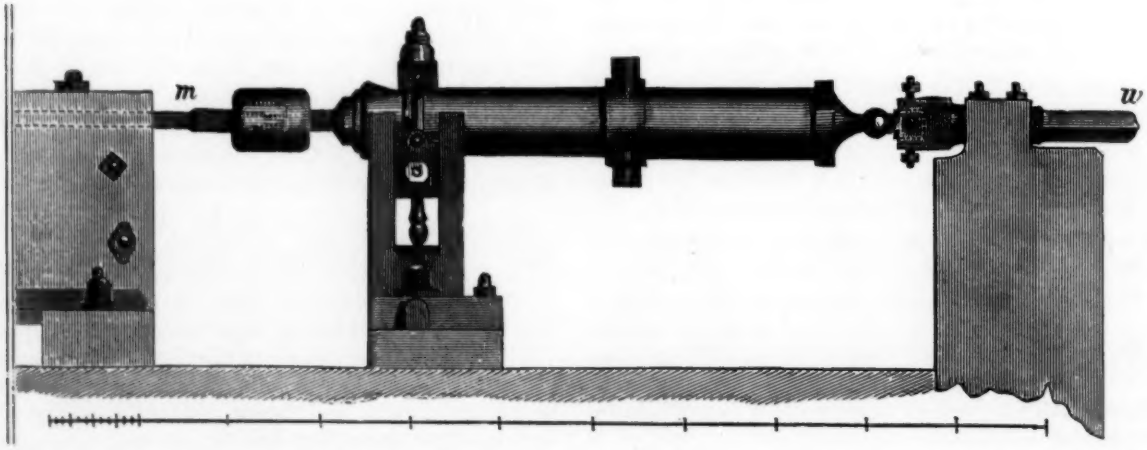

FIG. 3. "The machinery used in the experiments. The cannon is seen fixed in the machine used for boring cannon. *W* is a strong iron bar . . . being united with machinery that is carried round by horses, causes the cannon to turn round its axis.

"*m* is a strong iron bar, to the end of which the blunt borer is fixed; which, by being forced against the bottom of the bore of the short hollow cylinder that remains connected by a small cylindrical neck to the end of the cannon, is used in generating Heat by friction."

of heat by compression was by no means confined to gaseous bodies. For example, a piece of iron made red hot by hammering could not be heated a second time in that way unless it was meanwhile introduced into a fire. This fact was explained by supposing that the caloric which had been pressed out of it by the percussion was recovered in the fire.

Among the most famous experiments conducted for the purpose of disproving the materiality of heat were those of Count Rumford (11) on the "Sources of Heat which is Excited by Friction." The main outlines of these experiments, which have been described many times, can be deduced from the figure and caption from the paper cited above which is reproduced in figure 3. Rumford realized that such an elaborate experiment carried out in the arsenal at Munich might not have its importance recognized unless it could be repeated easily elsewhere. He therefore designed, some years later, a much simpler apparatus for demonstrating the production of heat by friction (6). This is shown in figure 4, which Rumford describes as follows:

> Let *A* be the vertical section of a brass rod which is . . . fastened in an upright position on a stout block, *B;* it is provided at its upper end with a massive hemisphere of the same metal, three and a half inches in diameter. *C* is a similar rod, likewise vertical, to the lower end of which is fastened a similar hemisphere. . . . *D* is the vertical section of a globular metallic vessel twelve inches in diameter. . . . The rod *A* goes through a hole in the bottom of the vessel, is soldered into the vessel, and serves as a support to keep it in its proper position.
>
> The centre of the ball, made up of the two hemispheres which lie the one upon the other, is in the centre of the globular vessel, so that, if the vessel is filled with water, the water covers the ball as well as a part of each of the brass rods.
>
> If now the hemispheres be pressed strongly together, and at the same time the rod *C* be turned, by some means or other, about its axis, a very considerable quantity of heat is generated by means of the friction which takes place between the flat surfaces of the two hemispheres. . . . The source of the heat which is generated by this apparatus is *inexhaustible*. As long as the rod *C* is turned about its axis, so long will heat be produced by the apparatus, and always in the same amount. If the globe-shaped vessel *D* is filled with water, this water becomes hotter and hotter, and finally begins to boil. . . .
>
> Whence comes this heat? This is the contested point, to determine which was the real aim of the experiment.
>
> It is certain that it comes neither from the decomposition of the water nor from the decomposition of the air. Various experiments on this point, which I have described at length in my memoir in the Philosophical Transactions, are more than sufficient to establish this fact beyond doubt.
>
> Just as little does it come from a change in the capacity of heat brought about by friction in the metal of which the hemispheres are composed. This is shown, first, by the continuance and uniformity of the production of the heat; and secondly, by an experiment bearing directly on this point, by which I am convinced that not the slightest change had taken place in the capacity of the metal for heat.
>
> Just as little does it come from the rods which are attached to the hemispheres, for these rods were al-

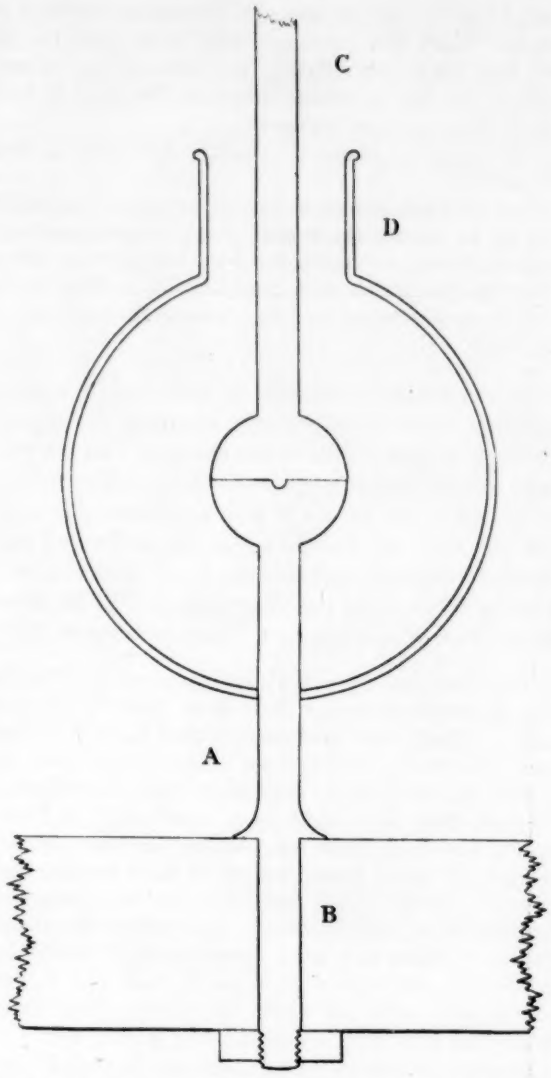

FIG. 4. Count Rumford's simplified apparatus for demonstrating the production of heat by friction.

ways warm, the hemispheres communicating heat to them. Much less could this heat come from the air or the water immediately surrounding the hemispheres, for the apparatus communicated heat to both these fluids without cessation.

Whence, then, came this heat? And what is heat actually?

I must confess that it has always been impossible for me to explain the results of such experiments except by taking refuge in the very old doctrine which rests on the supposition that heat is nothing but a vibratory motion taking place among the particles of the body.

It has become common, in histories of science, to credit Count Rumford with founding the modern concept of heat. This is not the case. In the passage quoted just above, we find him "taking refuge in the very old" theory of heat as a form of energy. He did not even consider that he performed particularly original experiments in his generation of heat by friction, for on November 8, 1797 he wrote from Munich to Professor Pictet in Geneva (12).

The results of my Experiments seem to me to prove to a demonstration that there is no such thing as an *Igneous fluid*, and consequently that Caloric has no real existence. You must not however raise your expectations too high respecting my experiments. Though they were made on a large scale, and conducted with care, there was nothing very new or very remarkable about them; and as to their results, they proved only this single fact—that the heat generated by friction is *inexhaustible*, even when the bodies rubbed together are, to all appearances, perfectly insulated, or put into a situation in which it is evidently impossible for them to receive from other bodies the heat they are continually giving off.

It appears to me that that which any insulated body, or system of bodies can continue to give off without limitation, cannot be a *material substance*.

Count Rumford made the first measurement of the mechanical equivalent of heat. In connection with this subject one immediately thinks of James Prescott Joule who wrote (13):

One of the most important parts of Count Rumford's paper, though one to which little attention has hitherto been paid, is that in which he makes an estimate of the quantity of mechanical force required to produce a certain amount of heat. . . . According to Count Rumford's experiment . . . the heat required to raise 1 lb of water 1° will be equivalent to the force represented by 1034 foot-pounds. This result is not very widely different from that which I have deduced from my own experiments related in this paper, viz. 772 foot-pounds; and it must be observed that the excess of Count Rumford's equivalent is just such as might have been anticipated from the circumstance, which he himself mentions, that "no estimate was made of the heat accumulated in the wooden box, nor of that dispersed during the experiment."

CONCLUSION

Count Rumford published over seventy papers on a wide variety of scientific subjects and throughout he makes constant reference to his belief in the energy theory of heat. Most of these references require so much background information that they have not been mentioned in this review where only his more elaborate and direct experiments have been discussed. For a true understanding of his contemporary preeminence as an authority on the theory of heat one must study his original work, which is permeated with an enthusiasm illustrated when he wrote (6):

To engage in experiments on heat was always one of my most agreeable employments. This subject had already begun to excite my attention, when, in my seventeenth year, I read Boerhave's admirable Treatise on Fire. Subsequently, indeed, I was often prevented by other matters from devoting my attention to it, but whenever I could snatch a moment I returned to it anew, and always with increased interest. Even now this object of my speculations is so present to my mind, however busy I may be with other affairs, that everything taking place before my eyes, having the slightest bearing upon it, immediately excites my curiosity and attracts my attention.

This habit of many years' standing, by force of which I seize with the greatest eagerness, and endeavour to investigate, each and every phenomenon related even in the slightest manner to heat and its operations which comes to my knowledge, has suggested to me almost all the experiments that I have performed with reference to this subject.

REFERENCES

1. Tyndall, John. 1871. Heat considered as a mode of motion, 39. N. Y., Appleton.
2. Rumford, Count. 1804. An inquiry concerning the nature of heat and the mode of its communication. *Phil. Trans.* **94**: 77–182.
3. ——. 1805. An account of some new experiments on the temperature of water at its maximum density. *Nicholson's Journal* **11**: 225–235.
4. ——. 1798. Of the propagation of heat in various substances, Essay VIII. London, Cadel and Davies.
5. ——. 1786. New experiments upon heat. *Phil. Trans.* **76**: 273–304.
6. ——. 1804. Mémoires sur la chaleur. Paris.

7. ——. 1799. On the propagation of heat in fluids. *Ann. der Phys.* **1**: 436–463.

8. ——. 1807. Recherches sur le progrés lent du mélange spontané de certains liquides disposés à s'unir chimiquement les uns avec les autres. *Mem. de l'Inst. National de France* **8**: 109–115.

9. EINSTEIN, ALBERT. 1905. Über die von der molekularkinetischen theorie der wärme geforderte bewegung von in ruhen den flüssigkeiten suspendierten teilchen. *Ann. der Phys.* **17**: 549–560.

10. RUMFORD, COUNT. 1799. An inquiry concerning the weight ascribed to heat. *Phil. Mag.* **5**: 162–174.

11. ——. 1798. An inquiry concerning the source of the heat which is excited by friction. *Phil. Trans.* **88**: 80–102.

12. The American Academy of Arts and Sciences has a collection of copies of letters from Rumford to Pictet.

13. JOULE, JAMES PRESCOTT. 1850. On the mechanical equivalent of heat. *Phil. Trans.*, 61–82.

A THEORY OF PROTOPLASMIC STREAMING

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INTRACELLULAR motion of cytoplasm has occupied the attention of biologists ever since Corti (1776) first reported it. Investigators have, through a variety of theories, attempted to suggest a force which could account for the motion. Almost every known form of energy has been proposed to account for protoplasmic streaming. Forces produced by changes in surface tension, hydration, osmotic pressure, and fluidity of portions of protoplasm feature in some of the theories. Other theories utilize magnetic, electrophoretic, electroosmotic, and contractile forces.

The level of generality of these theories differs greatly. The surface tension theory arises out of studies of amoeboid motion and is apparently inapplicable to other forms of streaming. The hydration theory appears to be restricted to fungi. The electrokinetic and the contractile theories are possibly applicable to all forms of intracellular motion. It has been argued that cyclosis in the plant cell and streaming of the slime mold are based on entirely different mechanisms (Heilbrunn, 1943). Since we have as yet no definite knowledge of the mechanisms of any one of the forms of motion, such a view is a matter of preference. It is a basic principle in the scientific method to prefer the simplest and most general theory. Hence, the ability of a theory to explain a wide variety of forms of intracellular motion will be viewed in this discussion as an important criterion of its usefulness.

A detailed critical examination of all the forces mentioned previously will not be attempted here. None is sufficiently broad to explain all we know of intracellular motion. Serious criticisms have been levelled at each one (Seifriz, 1943).

The only biological motion-producing force of which we have some detailed knowledge is contractility of the muscle fiber. We are becoming acquainted with some of the metabolic mechanisms by which energy stored in foodstuffs is channelled to certain protein molecules. These apparently are able to use chemical energy to contract and thus exert a force. It seems plausible, therefore, to explore fully the possibilities of a contractile theory of intracellular motion.

Contractility as a motile force was advocated by Corti as early as 1774 and has since been considered by numerous workers (Corti, 1776; Heidenhain, 1911; Kühne, 1864; Pfeffer, 1906; Rachevsky, 1939; Reichert, 1866; Verworn, 1892, 1899; Vouk, 1913). Pfeffer applied the theory to the Myxomycetes and hypothecated a rhythmic contractility to account for the reversal of flow often found in these organisms. Seifriz (1937) took time-lapse pictures of a pseudopod of *Physarum polycephalum* and observed rhythmic contractions and expansions of the pseudopod. Unfortunately, this observation does not indicate whether the contraction is a cause or an effect of protoplasmic flow. It is generally assumed that the contraction occurs in the cortical region. The time-lapse experiment suggests that it occurs in the periphery of the pseudopod. Detailed observation of the lines of flow within a pseudopod reveals that this cannot be so, for they seldom flow all in the same direction (fig. 1). If the pseudopod as a whole contracts, it does so because the summation of the outgoing lines of flow is larger than that of the ingoing lines. Kamiya's (1942) observations of polyrhythmicity indicate the same thing, namely, that the contraction of a pseudopod is the result of the integration of several rhythms. We, therefore, have to look for a contractile force which is in much more intimate contact with the stream, thus

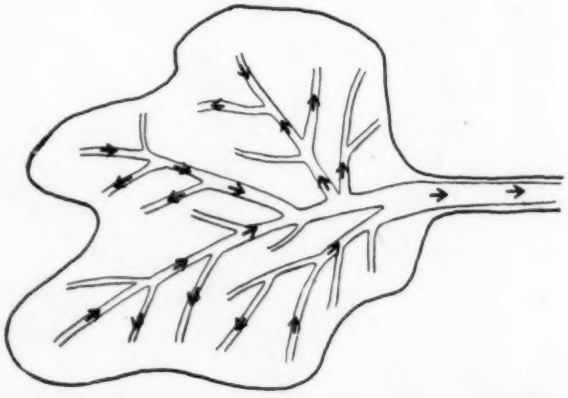

FIG. 1. Diagram of pseudopodium of *Physarum polycephalum* showing lack of correlation between direction of various streams.

permitting the observed autonomy of each line of flow. Seifriz suggests that such a force may exist in the form of a peristaltic wave of contraction located in the cortical region surrounding the stream. Since no such peristaltic contraction has been observed, its amplitude would have to be of submicroscopic dimensions. Even this modification of the contractility theory meets with serious objections. A plasmodium has recently been described (Nauss, 1949) which has the amazing characteristic of possessing several parallel lines of flow in opposite directions. As many as eleven such lines of flow have been observed to occur. A peristaltic contraction theory might be salvaged if we could demonstrate a cortical region surrounding each line of flow. But no such region exists! This can be demonstrated by observing the free interchange of large droplets and granules between adjacent streams.

These objections do not rule out the possibility that contractility may be the motile force of streaming. One can postulate that the region of contraction is located in the stream itself. This is a very attractive possibility, for it tends to side-step all the objections levelled at the contractility theories and is in keeping with the autonomous behavior exhibited by protoplasmic flow.

To shape this theory into a more definite mechanism, the following tentative picture is suggested:

1. The motile force is located in certain contractile protein constituents of the stream. These proteins are somewhat elongated and are parallel to the line of flow.

2. The proteins are locally and momentarily attached to a surface external to the stream, by a mobile and relatively weak bond, possibly a hydrogen bond. This surface will be referred to as the *reaction surface*.

Figure 2 shows how these two factors may be combined to produce motion.

The scheme in figure 2 is one of many alternative possibilities. The bond may not shift from the anterior region of the molecule to the posterior, but may travel down the molecule in several stages. More than one bond may be moving down the molecule at any one time. The molecules may be loosely connected end to end so that a series of bonds travels down a ribbon of connected molecules.

3. The above theory involves the following implicit assumption. The scheme in figure 2 says that a molecule will move forward (in the direction of the stream) if it forms a bond at the anterior end before or during the contracting phase

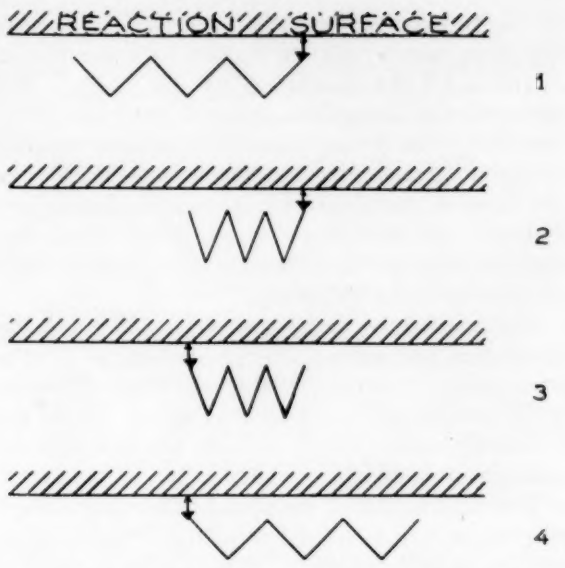

FIG. 2. (1) Protein molecule in extended form. Bond at anterior end of molecule. (2) Protein molecule in contracted form. (3) Shifting of bond from anterior to posterior end. (4) Protein molecule in expanded form.

and at the posterior end before or during the expanding phase. Let us say that the molecules which form bonds in this way so as to contribute to the motion are *in phase* and those which may form bonds so as to oppose the motion are *out of phase*. It is clear that streaming can occur only when the number of molecules in phase exceeds those out of phase. This cannot happen by chance alone and we must therefore postulate that some mechanism is present which sets the majority of stream proteins in phase. This mechanism must of course be intimately related to the mechanism responsible for the reversal of streaming.

The nature of the reaction surface may vary. In *Physarum polycephalum* a flowing region is always surrounded by a stationary cortical envelope (which probably provides the reaction surface). In the plasmodium previously described (Nauss, 1949), it is likely that the region may be the solid surface on which the plasmodium is resting, or possibly the slimy excretion which is so often found to occur between the slime mold and its substratum. This possibility finds substantiation in the fact that the organism has the faculty of sending out thin strands which seem to move *in toto* without any observable portion of stationary protoplasm surrounding it. The fact that each line of flow is always found to be in contact with the surface of the slide is consistent with the above view.

The reversal of streaming observed in some of the slime molds could be caused by a reversal in direction of the movement of the bonds. But since reversal is not a necessary adjunct of streaming, and since it can even be eliminated without cessation of streaming in *Physarum polycephalum* by caffeine (unpublished results of Seifriz and Pollack) and pressure (Pease, 1940), its mechanism should not be a serious consideration in a general theory of streaming.

The mechanism suggested does not make use of obscure principles. The phenomena of protein contraction (Astbury, 1940; and Szent-Györgyi, 1947) and shifting of bonds (Pauling, 1945) are commonly encountered in both animate and inanimate systems.

The contraction of the muscle fiber is generally considered to be the result of the contraction of certain muscle proteins. Szent-Györgyi and his co-workers believe that these proteins are the actin and the myosin fractions of muscle which, when combined in a complex, are capable of a remarkable degree of contraction in vitro.

The rapid shifting of bonds is frequently encountered in thixotropic sol-gel transformations both in situ and in vitro. The hydrogen bond is an ideal bond for such phenomena. According to Pauling (1945) "the hydrogen bond is the only strong and oriented intermolecular interaction which can come into operation quickly." He is of the opinion that the hydrogen bond will be found to feature importantly in muscle contraction and nerve conduction. It may therefore be concluded that the mechanism suggested above is based on phenomena quite commonly encountered.

Just as in muscular movement, the energy required could be utilized in the contraction or the relaxation phase. A small fraction of the energy required is probably used in formation and breaking as well as in the "driving" of the bonds.

The above mechanism has the potentiality of being applicable to all forms of intracellular motion. Streaming, cyclosis, and even the independent motion of protoplasmic bodies such as chloroplasts, nuclei, and chromosomes may all be carried out by the same mechanism. Certain forms of extracellular motion such as gliding movements may be explained by assuming that contractile proteins reside at the surface. Even muscle contraction might eventually be viewed as a special case of protoplasmic motion based on the same basic principle of protein contractility but having become specialized through the course of evolution.

Scarth (1942) connects the protoplasmic surface film (ectoplast) with the nuclear, chloroplast, and vacuolar membranes by a cytoplasmic stream. He considers that all these membranes have the same basic properties as the cytoplasmic stream. Thus, the migration of nuclei and chloroplasts may be caused by contractile proteins residing in the membranes of these bodies. The movement of the paternal nucleus down the mycelium of a fungus during the process of diploidisation is strongly suggestive of autonomous nuclear movement. Similarly, it is much simpler to view the migration of randomly dispersed chloroplasts toward a light source as caused by their independent motion than to assume that some very intricate movement of the cytoplasm brings about the same result. The autonomous movement of the vacuolar membrane has also been observed (Seifriz, 1943).

Belar (1929) has noticed "tractile fibers" (Zugfasern) which he claims are associated with the kinetochore of the chromosome during the metaphase and anaphase of mitosis. He considers these fibers to be fluid and to be responsible for the motion of chromosomes. He envisages a gliding action of his tractile fiber over the continuous spindle fiber. Numerous other observations have been made which strongly indicate that the movement of the chromosomes during mitosis is autonomous (Bleier, 1930, 1930; Frey-Wyssling, 1938; Metz, 1933, 1936; Schrader, 1944; Scott, 1936). The concept of chromosomal autonomy as well as Belar's "tractile fibers" fit nicely into the above picture.

The independent motion of cytoplasmic strands in the vacuole of plant cells is highly suggestive of the autonomy of streaming. Since the strand is often observed to move *in toto*, we would have to hypothecate the existence of reaction surfaces in the main body of the cytoplasm causing the strand to be "paid out" at one end and "reeled in" at the other. Scarth's observation that streaming vacuolar strands often sag and then become taut again is consistent with this picture.

It has been observed that in the amoeba the granules in the outgoing pseudopod start to move before the granules in the rear portion of the amoeba. This observation presents a serious objection to the classical theories of amoeboid motion but offers no obstacle to an autonomous theory. In fact it seems quite plausible to assume that pseudopod formation is intimately related to or

even caused by the activity of the stream proteins of the pseudopod.

On the basis of his work on cells of early amphibian embryos, Holtfreter (1946) concluded that amoeboid motility is due to autonomous expansions and contractions of the outermost layer (plasmalemma) of these cells. Since he showed that small pieces of the plasmalemma are capable of independent motion, the contractions and expansions must be parallel to the surface rather than perpendicular. This picture of motion caused by autonomous expansion and contraction at an interface seems to be in general agreement with the theory presented in this paper.

It is at present premature to build a speculative superstructure on these hypothetical foundations. We would have to know much more about the mechanism of bonding and contraction to show how these phenomena are regulated over a unirhythmic portion of the stream.

Because of the technical difficulties inherent in the study of the structure and behavior of proteins in the cell, it is unlikely that a single experiment can at present be designed to validate this theory of streaming. It is more likely that, if the theory is correct, its validity will gradually become more and more convincing as increasingly numerous observations will be fitted into its general framework. It is with this end in view that this paper has been written.

To summarize, the purpose of this paper is to:

1. Draw attention to the possibility that the force responsible for streaming is exerted by the stream itself.

2. Suggest a possible mechanism, based on the well-known principles of protein contraction and the shifting of bonds by which motion may be obtained.

3. Point out that although there are few data available to put the theory on a secure foundation, the data are nevertheless consistent with the theory.

4. Show that the framework of the theory is sufficiently broad to include all forms of intracellular motion as well as some forms of cellular, i.e. translatory, motion.

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REFERENCES

ASTBURY, W. T., AND SYLVIA DIKINSON. 1940. X-ray studies of the molecular structure of myosin. *Proc. Roy. Soc. London* B, **129**: 307–332.

BELAR, K. 1929. Beiträge zur Kausalanalyse der Mitose. III. *Ztschr. Zellf.* **10**: 73–134.

BLEIER, H. 1930. Experimentell-zytologische Untersuchungen. I. *Ztschr. Zellf.* **11**: 218–236.

—— 1930. Untersuchungen über das Verhalten der verschiedenen Komponenten bei der Reductionsteilung von Bastarden. *Cellule* **40**: 85–144.

CORTI, B. 1776. Sur la circulation d'un fluide, decouverte en diverses plantes. *Rosier Observ. Sur la Physique, Sur l'Histoire nat. etc.* **8**: 232–286.

FREY-WYSSLING, A. 1938. Submikroskopische Morphologie des Protoplasmas und seiner Derivate. Berlin, Borntraeger.

HEIDENHAIN, M. 1911. Plasma und Zelle. Jena, Fisher.

HEILBRUNN, L. V. 1943. An outline of general physiology. Phila., Saunders.

HOLTFRETER, J. 1946. Structure, motility and locomotion in isolated embryonic amphibian cells. *Jour. Morph.* **79**: 27–62.

KAMIYA, N. 1942. Physical aspects of protoplasmic streaming. The structure of protoplasm, 199–244. Ames, Iowa State College.

KÜHNE, W. 1864. Untersuchungen über das Protoplasma und die Contractilität. Leipzig.

METZ, C. W. 1933. Monocentric mitosis with segregation of chromosomes in *Sciara* and its bearing on the mechanism of mitosis. *Biol. Bull.* **64**: 333–347.

—— 1936. Factors influencing chromosome movements in mitosis. *Cytologia* **7**: 219–231.

NAUSS, R. N. 1949. Reticulomyxafilosa gen. et sp. nov., a primitive plasmodium. *Bull. Torrey Bot. Club* **76**: 161–173.

PAULING, L. 1945. The nature of the chemical bond, 431. Cornell Univ. Press.

PEASE, D. 1940. Hydrostatic pressure effects upon protoplasmic streaming in plasmodium. *Jour. Cell. and Comp. Physiol.* **16**(3): 361–372.

PFEFFER, W. 1906. The physiology of plants, English ed. 3v. Oxford, Clarendon.

RACHEVSKY, N. 1939. Some remarks on the movement of chromosomes during cell division. *Bull. Math. Biophys.* **1**: 47–62.

REICHERT, C. B. 1866. Über die Saftströmung mit Rücksicht auf die Contractilitätsfrage. *Monatsber. Kön. Preuss. Akad. Wiss.* 318–313.

SCARTH, G. W. 1942. Structural differentiation of cytoplasm. The structure of protoplasm, 99–107. Ames, Iowa State College.

SCHRADER, F. 1944. Mitosis. Columbia Univ. Press.

SCOTT, A. C. 1936. Haploidy and aberrant spermatogenesis in a coleopteran, *Micromalthus debilis* Le Conte. *Jour. Morph.* **59**: 485–515.

SEIFRIZ, W. 1937. A theory of protoplasmic streaming. *Science* **86**: 397–398.

—— 1943. Protoplasmic streaming. *Bot. Rev.* **9**: 41–123.

SZENT-GYÖRGYI, A. 1947. Chemistry of muscular contraction. N. Y., Academic Press.

VERWORN, M. 1892. Die Bewegungen der lebendigen Substantz. Untersuchung der Kontractionserscheinungen. Jena, Fisher.

—— 1899. General physiology. London, Macmillan.

VOUK, V. 1913. Untersuchungen über die Bewegungen der Plasmodien II. Studien über die Protoplasmaströmung. *Denkschr. Akad. Wiss. Wien. Math. Nat.* **88**: 652–692.

PREVENTIVE PSYCHIATRY

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HUMANITY today faces only one basic problem: man's hostility to man. This problem has only one basic solution: the proper rearing of children to emotional maturity.

I. INTRODUCTION

Preventive Psychiatry is a new born infant. It is the child of Dynamic Psychiatry and Sociology, with close blood relations to the other psychological and social sciences. It is lusty and it is legitimate. The marriage, however, was something of a shotgun affair, and father Mars makes it plain that his atomic gun is still handy and his trigger finger is still itchy. The family of sciences hope that the new child may grow up to pacify the old man.

Preventive psychiatry is essentially one of mankind's attempts to adapt to the changing conditions of life. Tapping the atom multiplies by about forty million the amount of physical power at our disposal. We must adapt to this incomprehensibly enormous potential if we are to survive.

Human behavior, specifically human social behavior, the behavior of people to each other, has become the key to survival or to total destruction. In the minds of men begin not only wars but humanity's most glowing joys, and its shattering miseries. This has always been known, but never fully realized. Now it is painfully obvious. And so there is a great turning to the psychological and social sciences to save us. The specialist in motivation is asked to help people behave toward each other in ways which will enable them to live together on this planet. Individuals come with personal problems—their feelings, their child's behavior, their marriages, their jobs. They swamp the qualified specialists and they swamp the unqualified and the outright charlatans (16).[1] Not only individuals come with their own personal problems, but groups and organizations, such as Parent-Teacher associations, schools, industry, and government. During and after the First World War, the importance of economics was recognized in international negotiations along with military and diplomatic knowledge and procedures. Now psychologic and sociologic knowledge and techniques are increasingly recognized as necessary (4).

Modern dynamic psychiatry is ready, as a basic science, to meet this challenge. Why it is ready is a fine question for the historian. The fact is that the past fifty years, which saw the development of atomic physics, also saw the development of dynamic psychiatry. Penetration by science of the nature of the atom and the forces within, has been parallelled by similar penetration of the nature of the mind and the forces within its depths. Of course the depths of human motivation have long been known to the poet, writer, religious leader, and philosopher—to say nothing of the salesman. But only now, with appropriate technics of study, is it part of Science—that is, part of established knowledge which is organized, teachable, applicable.

Our minds are powered by the biologic drives and processes of our bodies and also by the conditioning influences of our childhood and by the circumstances of life to which we must adapt. Thus dynamic psychiatry stands between biology and sociology. It deals not only with the deep unconscious processes but also with the total personality, with all relevant motivations and determinants of behavior, external as well as internal (1).

II. THE NEED

With Freud, the focus of psychiatry shifted from the custody of the psychoses, to exploration of the emotional forces behind the neuroses. The neuroses meant spells of anxiety, nightmares, fainting attacks, varying degrees of impotence and frigidity, periods of depression, disturbing ideas which could not be banished, and so on. These symptoms were found to derive from loves, hates, ambitions, fears, and other everyday feelings which had got out of kilter. The emotional forces which produced neurotic symptoms turned out to be the emotional forces by which we all live. The focus broadened. These same forces, whether conscious and overt, or hidden from the

[1] The numbers in parentheses indicate the references at the end of the article.

person himself, were seen to have many other outcomes. As Freud put it, the study of these forces was leading out of the Mediterranean of neurotic symptoms into the great Atlantic of personality make-up and human behavior.

Psychoanalysis began with a study of "conversion hysteria," a misleading term for physical symptoms of emotional origin. The first patient Freud reported was a young girl with a nervous cough, nervous nausea, and asthma attacks. Psychosomatic medicine (17, 2*a*) is a whole new field devoted to exploring the role of emotions in physical disorders. These are considerable in heart and circulatory disturbances, in stomach ulcers, asthma, headaches, and other conditions (2). Looking further, emotional forces underlie delinquency and crime, divorces, alcoholism, and many a wrecked career, and mismanaged life (2).

It is evident that the personal problems which psychiatry faces must be numbered, in this country alone, in the tens of millions (10*a*). There are as many beds for psychotics alone as there are all other hospital beds put together—over one-half million. Ninety-seven per cent of these beds for psychotics are in government institutions. There are another half million psychotics not in institutions, making a total of about a million. This million, however, is now seen to be the very least of the problem—it represents only the extreme of breakdown. It is difficult to estimate the number of persons with classic neurotic symptoms severe enough to make help urgent. The figure derived from Selective Service examinations is five million (13). One child of every twenty born will spend time in a mental institution. One in ten will be incapacitated for some period by lesser breakdown.

Even this is but a small fraction of the over-all problem. Physicians judge that from one-third to two-thirds of the practice of medicine deals with complaints which arise basically from emotional tensions, from the stresses and strains of living. We have three million alcoholics, over a half million of them so advanced as to show deterioration. Accidents kill nearly one hundred thousand and injure nearly ten million a year, and we know these are largely unconsciously motivated. About seven million of our citizens have criminal records. One million six hundred and fifty thousand crimes are committed yearly. One and a quarter million children pass through our juvenile courts annually; it is estimated that three million children are in need of psychiatric help.

The undercurrent of emotional difficulties is reflected in the half million divorces against the one and one-half million marriages yearly. One marriage in three ends in divorce; another one in three ends in legal or unofficial separation, and this only suggests what tensions and unhappiness our lasting marriages sustain, and why our children so rarely grow up properly to be good marital partners, parents, and citizens.

This does not exhaust the psychiatric problems. It does not count the needless failures in career, the needless daily cruelties, and the emotional sufferings which never get into statistics. But it shows that psychiatry deals with such vast numbers that it must be basically *preventive*. There can never be enough psychiatrists to *treat*. Only through prevention can psychiatry be effective.

These figures show also that the psychiatrist cannot do the preventing alone. The man who comes to the office, the mother who brings her child to a clinic, the woman who seeks advice on a radio program, all have problems which reach into our national life. This is true even of functional medical conditions. Let us take heart disease, now by far our greatest killer, Much of it is hypertensive. "High blood pressure," to quote a great internist, the late David Riesman,

> is not only a disease of the individual—it is a disease of American life. It extends to all our communal doings; it is reflected in the tension under which every individual in America lives. . . . [The] causes are, I believe, connected with our striving for wealth. We have created false standards, have deprived ourselves of peace and leisure, and have lost the art of living wisely (11).

Thus modern dynamic psychiatry must deal with biology and with sociology. The sociologic problems go beyond the personal sufferings we have listed above. Even these fade to lesser import before the vista of terror, tyranny, and war. These latter horrors have no other motivations than those we have discussed. Rage, pent-up, can generate headaches, anxiety, palpitations, insomnia, stomach disorders. Rage, vented, can make cruelty, even murder. Beyond the law it is crime or obvious pathology. But within the law, sublimated through a good intellect, it can be organized. It can attract the hate in others. It can disrupt adaptation through reason and cooperation. It can seek to sanction the primitive method of adaptation by force, the animal method of meeting problems by fight and flight (9). It can generate tensions between groups. It can make social conflict and war. The same motiva-

tions which make the various personal psychiatric conditions are what make men mismanage social affairs. Groping human beings—unable to manage their own lives, it is small wonder they cannot manage the lives of others. Science has found a way out—a way that supports the insights of the great religions. If science lives, men will learn, in not too many generations.

III. THE DYNAMICS

Clearly, then, the goal of dynamic psychiatry goes far beyond the problems of individual emotional disorder. It reaches to the roots of national and world tensions and antagonisms. It does this because it deals more directly than any other discipline with the basic motivations and feelings, conscious and unconscious, which underlie these failures of cooperation and adjustment.

These disorders are found regularly to derive at bottom from malformations of personality, or put another way, from failures to achieve emotional maturity.

All forms of neurotic behavior reflect warped patterns of reaction which have persisted from childhood. Here is a man who has not outgrown his childish dependence, although all he feels is a sense of inferiority which fills him with a rage he must constantly hide from himself and others. Here is a girl who was too long the little queen in her family and now is in a chronic frustrated rage because life does not baby her as family did. Here is a man too harshly reared, who hates all authorities over him, and, identifying with them, is tyrannical to all who come under him.

By studying an endless series of individuals of all ages, we see the normal course of the human biologic and emotional development. We also see what people would be like if their development were not impaired, and how they progress on this course if they are treated successfully.

The child he once was lives in every adult, and so do the parents or others who reared him. If this child was well reared he will not dominate the adult's mature feelings and behavior, but will add zest to them. The adult will be able to satisfy his childish needs in socially acceptable ways, and still be predominantly (1) independent, (2) able to give love and be interested in others, (3) not egocentrically competitive, (4) at peace with a conscience which is social and favors mature behavior, (5) able to use his sexual drives for mating and binding his family, (6) realistic and not warped in his views by his childish feelings, (7) adaptable because not unduly fixed in childhood patterns, and (8) free from hate and hostility, although able to be aggressive in defense and for unselfish constructive purpose. Briefly, the mature adult achieves toward humanity the attitudes of the good, strong, loving parent toward his child and mate.

If a reasonable fraction of our population achieved that enjoyment of productive, responsible interest in their families, work, friends, and society, which is the mark of maturity, most of our problems would be readily solved. Our problems are not difficult technically, but because they involve dealing with adults who are immature, and hence anxious, egocentric, grasping, and hostile.

Most adults unconsciously feel that the only pleasures are those they dreamed of in childhood, especially to be care-free and cared-for. Hence they do not enjoy the use of their adult powers for productivity and responsibility. Yet, to live they must be responsible and productive. They rebel. They cannot realize the childhood dreams and have not learned the pleasures of exercising the powers of maturity. They should enjoy both, but they enjoy neither; and hostility piles up. Hostility is humanity's central problem. It is central in the psychoses and neuroses—central in personal living, and central on the national and international scene. The ultimate solution rests with rearing enough mature adults.

IV. THE GOAL

It is a finding of revolutionary importance that a mature adult is basically a constructive, responsible, loving person. It means that nature is on our side in this struggle to adapt to each other. We have the power and technology to adapt to nature. Now the threat is from within man himself. But this we find derives from the mistreatment of children. Rear them properly and one gets good parents and citizens. Here is a basis in established knowledge of man's psychobiologic development for a scientifically grounded ethics and philosophy of life (14*a*, 10).

No matter how clever people are we cannot make a United Nations work if the infantile motivations are too strong. Conversely, with mature adults, most problems of living together are not difficult. Thus psychiatry is becoming more and more concerned with the positive side of our psychologic development—with mental health. Mental health depends (1) upon proper emotional development and (2) upon proper conditions of living. These conditions include not only the physical environment but also the ideologies and mores which influence us so subtly and per-

vasively, and which are so often neurotic, that is childish.

Emotional maturity is the basis of (1) mental health, (2) morality and ethics, and (3) social cooperation; and (4) it is the only healthy long range answer to mankind's central problem, namely, man's inhumanity to man. What helps children develop normally toward emotional maturity, helps mankind make peace and brotherhood into realities (14).

V. APPLICATIONS

Preventive psychiatry seeks to contribute what it can to these goals in various ways.

As part of science it depends upon basic pure research. This is its greatest difficulty. Medicine rests on basic sciences, anatomy, physiology, pathology, and the like. Psychiatry has been a form of medical practice without a basic science. It sorely needs good research men and financial support for them to work.

With basic scientific knowledge it seeks to develop applications, just as engineering applies physics and chemistry. These applications are both long range and short range.

Increasingly the psychiatrist finds himself in a new role. His function as a therapist has become almost insignificant before the tens of millions who need treatment. He is called upon rather to supply the basic knowledge of the emotional life for others to use. He must work this out with the sociologist, anthropologist, political scientist. And he develops applications of these data only through other fields and organizations. Hence most developments in preventive psychiatry are multidisciplined.

Some short range applications have been made. During the war combat fatigue was greatly reduced in the infantry through John Appel and Beebe showing how proper leadership, periodic relief, and limitation of combat days could influence the breakdown.

Other applications are now being tested in consultation services to communities, schools, courts, and government, on national and international problems.

The Mental Health Division of the U. S. Public Health Service is operating demonstration or pilot clinics and teaching institutes (3, 15).

Lemkau (7), at Johns Hopkins, has shown how public health services can effectively include preventive psychiatry in their activities. Lindeman of Harvard, collaborating with social scientists, is making almost a block survey of mental health in the town of Wellesley, Mass. Lemkau has made one in part of Baltimore (8). Such projects have been done in Denmark also. There are committees on preventive psychiatry of the American Psychiatric Association, and of the Group for the Advancement of Psychiatry.

The largest development in this field is the new World Federation of Mental Health, with headquarters in Geneva, Switzerland. This organization is an outgrowth of the International Congress on Mental Health (5), held in London last summer under the auspices of the World Health Organization of the United Nations. The Congress itself received papers written by interdiscipline preparatory commissions. Five thousand specialists, meeting in small groups, contributed three hundred articles. These represent a cross-section of the thought of psychologic and social scientists throughout the world.

The Department of Preventive Psychiatry at the University of Pennsylvania represents an attempt to develop sustained research in this field, and to funnel basic research into channels of prevention, both short range and long range.

The long range goal of preventive psychiatry is to make better people. It is to help people live together and enjoy life. This can be achieved through the proper rearing of children. This means socializing the child gently and gradually, in ways that help his development and do not impair it. It means socializing him without antagonizing him. It may mean changing ideologies and mores (6).

This task means reaching parents and children at all levels. It means reaching children as children and as future parents. This can be done in part through the schools. Bullis (2*b*) is trying this on a large scale in the lower grades with his "Human Relations" classes. Elizabeth Force is developing similar methods. Ojemann seeks to "humanize" all courses dealing with behavior, and to introduce the analytic approach of understanding motivation. We are trying to develop a course at Swarthmore. Churches, too, are interested in utilizing advancing knowledge. There is some interest in industry (12). The students have met this approach, wherever it has been tried, with an enthusiasm which reveals the depth of their need for it. The essence of this approach is free discussion and understanding of human motivation. Besides its value in relieving anxiety and aiding the emotional development, it is showing its superiority over blind discipline in

dealing with infringements. Defacement of washrooms, for example, ceased after full discussion with the boys in a Delaware school of their motivations in doing it.

Most of the suffering in the world arises from human hostility, from man's inhumanity to man. With the advances of medicine, disease is now very largely a result of this hostility as it comes to expression as war, poverty, neglect, corruption. Hostile people are made, not born. Case after case shows that hostility arises chiefly from defects in the emotional development—or from conditions of life which are made by others who have such defects. These defects are caused by faulty upbringing, by spoiling, deprivation, rejection, domination, cruelty, and the like. These are the sources of the evil in man. Emotionally mature people in reasonable circumstances are not hostile people.

In the end we can only make a better world by making better people. We have now solved this in theory and it is largely a practical problem—like turning theoretic knowledge of the atom into atomic power. It is a matter of avoiding the horrible cruelties to children, which, in these dark ages, are practised in ignorance and rationalized as "discipline" or "training" or "love." When children are treated as we want them to treat us, when children are so treated that they became emotionally mature adults and realize their true nature, mankind will emerge from the Kingdom of Evils and approach the Kingdom of Heaven. This, I believe, is the meaning of the scriptures. To add its bit toward furthering this goal is, as I see it, the long range goal of preventive psychiatry.

VI. SUMMARY

1. Psychiatry must be preventive for the disorders of motivations confronting it are of epidemiologic proportions. They number tens of millions and can never be appreciably helped by treatment.

2. The problems facing psychiatry are not only personal but also social—for the motivations which cause personal neurotic disorders also come to expression in social disorders, even war.

3. Preventive psychiatry must be multidisciplined—for though it can provide specialized scientific knowledge the applications must be engineered by other fields.

4. The core of prevention lies in the proper rearing of children—for it is known that neurosis is an expression of immaturity, and that the mature adult is a good parent, neighbor, and citizen. Rather than being driven to adult activities only by outer necessity and inner sense of duty, it is possible to enjoy both the exercise of the responsible productive adult powers and also the persisting childish wishes for playful pleasures.

Humanity today faces only one basic problem: Man's hostility to man. This problem has only one basic solution: The proper rearing of children to emotional maturity.

REFERENCES

1. Alexander, Franz. 1942. Our age of unreason. Phila., Lippincott.
2. Alexander, Franz, and Thomas French. 1946. Psychoanalytic therapy. N. Y., Ronald.
2a. Alexander, Franz, and Thomas French. 1948. Studies in psychosomatic medicine. N. Y., Ronald.
2b. Bullis, H. Edmund, and Emily E. O'Malley. 1948. Human relations in the classroom, courses 1 and 2. Delaware State Society for Mental Hygiene.
3. Felix, R. H. 1948. Mental hygiene and public health. *Amer. Jour. Orth.* **28**: 679–684.
4. Group for Advancement of Psychiatry, Report of Committee on Psychiatry and International Relations, Appendix I: 3.
5. International Congress on Mental Health. 1948 (12). Preparatory Commission Bulletin. Statement by International Preparatory Commission.
6. Kardiner, A. 1947. The frontiers of society. N. Y., Columbia Univ. Press.
7. Lemkau, Paul V. 1948. Opportunities for mental hygiene in public health. *Amer. Jour. Orth.* **28**: 670–678.
8. Lemkau, Paul, Christopher Tietze and Marcia Cooper. 1943. A survey of statistical studies on the prevalence and incidence of mental disorder in sample populations.
9. Levy, David M. 1948. Anti-nazis. Criteria of differentiation. *Psychiatry* **11** (2): 125–167.
10. McNeill, Harry. 1947. Freudians and Catholics. *The Commonweal* **46** (15): 350–353.
10a. Menninger, William C. 1948. Facts and statistics of significance for psychiatry. *Bull. Menninger Clinic* 12 (1).
11. Riesman, David. 1937. High blood pressure and longevity and other essays selected from the published writing of David Riesman, 149, Phila., Winston.
12. Rennie, Thomas A. C., Gladys Swackhamer and Luther E. Woodward. Toward industrial mental health. An historical review.
13. Rowntree, Leonard *et al.* 1945. Mental health and personality disorders in selective service registrants. *Jour. Amer. Med. Assn.* **128**: 1084–1087.
14. Saul, Leon J. 1947. Emotional maturity. Phila., Lippincott.
14a. ——. 1949. The individual's adjustment to society, *Psychoanalytic Quart.* **18**: (2): 191. (to appear shortly).
15. Stevenson, George B. 1948. Potentials for mental hygiene activities under public health authority, *Amer. Jour. Orth.* **28**: 684–690.
16. Stern, Edith. Where people take their troubles.
17. Weiss, Edward and O. Spurgeon English. 1943. Psychosomatic medicine. Phila., Saunders.

PELLAGRA—A DEFICIENCY DISEASE

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THE disease, pellagra, as we know it today was first described in 1735 by Caspar Casal, physician to Philip V of Spain, although his observations were not published until 1762. He referred to it as an infection and named the condition *mal de la rosa* because of the reddened skin associated with it. The French physician, François Thiery, published an account of the disease in a Parisian medical periodical in 1755 and stated that the disease was most prevalent at the season of the spring equinox and that the skin lesions must be located on the exposed surfaces of the hands or feet to justify the diagnosis. The modern name pellagra (*pelle*—skin, and *agra*—rough) was first recorded by Francesco Frapoli of Milan in 1771 who insisted that the lesions were precipitated by exposure to sunlight. The early symptoms of pellagra are weakness, lassitude, anorexia, and indigestion, followed by sore and ulcerated mouth and diarrhea. The typical dermatitis usually makes the diagnosis easy. Nervous symptoms also frequently occur.

In 1830 a census in Northern Italy revealed that in many areas 5 per cent of the population was suffering from pellagra. The disease was endemic throughout France from 1818 to 1880, but after the latter date the disease is said to have practically disappeared. The prevalence of pellagra in Spain, Italy, Rumania, and France, where maize formed a considerable portion of the diet, suggested to early students of the disease the presence of some toxic substance in corn. Experiences in the United States led to similar conclusions. The first cases were recognized in 1863 by Gray in Utica, New York, and by Tyler in Sommerville, Massachusetts. In 1907 several cases were discovered in the mental asylums of South Carolina and Alabama and an explosive outbreak in the southern part of the United States followed. By 1912, 30,000 cases, with a fatality rate of 40 per cent, had been described.

In 1908 Allbutt and Rollesten's *The System of Medicine* carried this statement: "Although pellagra, like ergotism, is associated with bad food and water, misery, and grinding labour, yet these conditions, which favor the disease, are not sufficient of themselves to produce it. . . . A more specific cause is necessary; and there is now no doubt that the immediate cause has been traced to bad maize." However, The Illinois Pellagra Commission (1910) concluded "pellagra is a disease due to infection with a living micro-organism of unknown nature." Similarly, the Thompson-McFadden Pellagra Commission (1914) summarized their report as follows "Pellagra is in all probability a specific infectious disease communicable from person to person by means at present unknown."

In 1914, at the request of Surgeon General Blue of the United States Public Health Service, a man who was destined to spend the remaining fifteen years of his life on pellagra studies entered the field. Joseph Goldberger, after a thorough and exhaustive study,[1] concluded that pellagra was not due to an infection but to a dietary deficiency. He was not the first to suggest the importance of diet, Funk had placed pellagra in the list of deficiency diseases and Vedder suggested a similarity between pellagra and known deficiency disease, but Goldberger was the first to offer convincing proof of the dietary origin of the disease. In short, Goldberger and his associates supplemented the diets of a group of children in several orphanages who were receiving biscuits, hominy grits, corn mush, syrup, molasses, gravy and sow belly, with fourteen ounces of milk per day, eggs, and fresh meat four days per week. Within a few weeks the disease disappeared and no new cases developed during the spring of 1915. Furthermore, a dozen volunteer convicts at Jackson, Mississippi, were placed on a typical pellagra-producing diet and developed typical symptoms in a period of five months.

[1] Goldberger, J., The etiology of pellagra, *Pub. Health Repts.* **29**: 1683, 1914; Goldberger, J., C. H. Waring and D. J. Willets, The prevention of pellagra. A test of diet among institutional inmates, *Pub. Health Repts.*, **30**: 3117, 1915; Goldberger, J., and G. A. Wheeler, Experimental pellagra in the human subject brought about by a restricted diet, *Pub. Health Repts.* **30**: 3336, 1915.

In spite of these very significant demonstrations, many students of the problem were skeptical because Goldberger had not produced the typical explosive endemic disease and because the workers had not been able to place their finger on a specific factor.

Attempts to ascertain more precisely the factors involved were given stimulus when Chittenden and Underhill at Yale reported in 1917 that dogs fed on a diet of crackers made from milled wheat flour, boiled peas, and vegetable fat developed diarrhea, loss of appetite, and ulceration of the oral mucous membranes. The symptoms promptly disappeared when fresh meat was supplied. In 1922 Wheeler, Goldberger, and Blackstock [2] concluded that the condition described by Chittenden was identical with canine blacktongue, a disease in dogs so named because of the necrotic areas on the tongue; and later these workers [3] were able to produce blacktongue in dogs on diets similar to those eaten by humans in pellagrous areas. Yeast was found to be active in preventing blacktongue in dogs and the yeast remained active after autoclaving. The factor was therefore associated with the more heat-stable fraction of the B complex which later came to be known as vitamin B_2.

In 1926 Goldberger and Lillie [4] produced a pellagra-like condition in rats fed a diet deficient in the B Complex but supplemented with an alcoholic extract of corn. They concluded that although it was highly probable that the pellagra-like condition in the rat was the analogue of pellagra in man, additional evidence was necessary to establish the relationship beyond doubt. We know today that this condition is not related to pellagra and that the dermatitis observed was due mainly to a deficiency of vitamin B_6. Nevertheless, the growth-stimulating effect of various foods when added to this ration continued to be used as a measure of antipellagra activity.

By 1930, shortly after Goldberger's death, it was generally agreed that a well balanced diet containing milk, eggs, fresh vegetables, and meat would prevent the disease. However, a considerable portion of the population in the South did not eat such a diet owing to ignorance, poverty, or wilfullness, and pellagra continued as a scourge especially when the price of cotton dropped below the cost of production, and little food could be purchased except corn meal, molasses, salt pork, dried beans, and syrup obtained through the country store or commissary. After the disease developed, few patients would consume foods containing a significant quantity of the antipellagra factor. Yeast was recognized as one of the richest sources but even when it was provided free of charge by the American Red Cross wretched subjects refused to consume it. Controlled clinical studies were being introduced by Spies in Cleveland and Smith at Duke University. Using these techniques Spies and others [5] were able to demonstrate a significant reduction in the mortality rate among patients with severe pellagra if the prescribed amount of yeast was actually consumed. In spite of these results certain workers claimed as late as 1934 that dietary measures were of little value.

During the early thirties another valuable source of the antipellagra factor became available. In 1926 Minot and Murphy found calves liver, and shortly thereafter, an extract of liver, to be very effective in the treatment of pernicious anemia. Goldberger and Sebrell [6] found this extract to be active in the prevention of blacktongue in dogs and clinical workers found fairly large doses to be efficacious in the treatment of pellagra (reviewed by Sebrell [7]). At that time I was working in the Biochemical Laboratory in Cambridge, England, and Dr. Guha who was also working there found the liver extract which I had brought along for anemia studies to be very active in promoting growth in rats on a vitamin B_2 deficient diet. The growth response was undoubtedly due to the presence of riboflavin but the results convinced me that liver extract would be excellent material for studies on the isolation of the antipellagra factor. It is interesting to look back now and note that liver extract has been used for isolation work in the case of most of the B vitamins but the isolation of the true antipernicious anemia factor, vitamin B_{12}, was made only within the past year.

[2] Wheeler, G. A., J. Goldberger, and M. R. Blackstock, On the probable identity of the Chittenden-Underhill pellagra-like syndrome in dogs and blacktongue with report of necropsy findings in two cases of blacktongue, *Pub. Health Repts.* **37**: 1063, 1922.

[3] Goldberger, J., and G. A. Wheeler, Experimental blacktongue of dogs and its relation to pellagra, *Pub. Health Repts.* **43**: 172, 1928.

[4] Goldberger, J., and R. D. Lillie, A note on an experimental pellagra-like condition in the albino rat, *Pub. Health Repts.* **41**: 1025, 1926.

[5] Spies, T. D., The treatment of pellagra, *Jour. Amer. Med. Assn.* **104**: 1377, 1935.

[6] Goldberger, J., and W. H. Sebrell, The blacktongue preventive value of Minot's liver extract, *Pub. Health Repts.* **45**: 3064, 1930.

[7] Sebrell, W. H., Vitamins in relations to the prevention and treatment of pellagra, *Jour. Amer. Med. Assn.* **110**: 1665, 1938.

Chicks in which a dermatitis was developed by feeding a heated grain ration were used for the early isolation work. In the final studies, dogs fed a modified Goldberger diet were used. Finally in 1937 we [8] demonstrated that nicotinic acid was active in the cure of blacktongue and we isolated nicotinic acid amide from the liver concentrates. Unlike most vitamins that have been characterized, nicotinic acid was a well known compound. It had been prepared in 1867 by oxidation of nicotine. Funk and others had isolated nicotinic acid as early as 1912 from yeast concentrates which possessed anti-neuritic activity but the compound had not been related to pellagra.

The activity of nicotinic acid in the treatment of blacktongue suggested its therapeutic use in human pellagra and the first report of its successful use was made by Spies, Cooper, and Blankenhorn and by Foutes before the Central Society for Clinical Research in Chicago, November 5, 1937. Since that time it has been used in the treatment of hundreds of cases with excellent results. The value of nicotinic acid in the treatment of pellagra is dependent upon the fact that it is cheap and that it can be administered readily to very sick patients without encountering the difficulty usually described when attempts were made to use yeast.

Pure nicotinic acid was found to be inactive in the prevention of chick dermatitis [9] and the active factor was soon identified as pantothenic acid.[10] Owing to the similar properties of nicotinic acid and pantothenic acid both vitamins had been concentrated in the same fraction. The function of nicotinic acid was soon recognized since Warburg and von Euler had described nicotinic acid amide as a constituent part of coenzymes I and II which are important in the cellular metabolism of both plants and animals. It was also possible to show a significant decrease in the coenzyme I content of liver and muscle from animals suffering from nicotinic acid deficiency.[11]

From a nutritional point of view it was important to determine the distribution of nicotinic acid in our food supply, especially in corn, to see whether the amount was low compared to other foods. The most successful method for the estimation of this vitamin has been the microbiological assay with *Lactobacillus arabinosus* as the test organism. In general, we [12] found excellent correlation between the antipellagra potency of foods as listed by Sebrell and the nicotinic acid content.[13] Corn was found to contain 1–2 mg./100 grams while other cereal grains, especially whole wheat, contained 5–6 mg. This then was an obvious reason why people living largely on corn and patent flour were prone to develop pellagra. During the milling of wheat 80–90 per cent of the nicotinic acid was lost. However, a serious discrepancy occurred when milk and eggs were analyzed for nicotinic acid. They were found to be extremely low (0.1 mg./100 grams) and you will remember that Goldberger used milk and eggs in the treatment of pellagra in several different orphanages. This also raised a serious question regarding the nutrition of infants that are restricted almost exclusively to a milk diet. We attempted to study this problem in infants in 1940 but owing to the war situation it was impossible to use infants and we turned our attention to rats. Rats were placed on a diet low in nicotinic acid and also low in protein, which would make the diet somewhat comparable to that consumed by pellagrins. This was done by using a low level of casein (table 1) and adding 40 per cent of corn

TABLE 1

NIACIN-LOW PURIFIED RATION

Sucrose	77.8 gm.	Thiamine	0.2 mg./100 gm.
Casein	15 gm.	Riboflavin	0.3 "
Corn oil	3 gm.	Pyridoxine	0.25 "
Salt IV	4 gm.	Ca pantothenate	2.0 "
l-Cystine	0.2 gm.	Choline chloride	100.0 "
Halibut liver oil plus vitamin E and K—1 drop/rat/week		Inositol	10.0 "
		Biotin	0.01 "
		Folic acid	0.02 "

[8] Elvehjem, C. A., R. J. Madden, F. M. Strong, and D. W. Woolley, Relation of nicotinic acid and nicotinic acid amide to canine blacktongue, *Jour. Amer. Chem. Soc.* **59**: 1767, 1937; and The isolation and identification of the anti-blacktongue factor, *Jour. Biol. Chem.* **123**: 137, 1938.

[9] Mickelsen, O., H. A. Waisman, and C. A. Elvehjem, The inactivity of nicotinic acid in chick dermatitis, *Jour. Biol. Chem.* **124**: 313, 1938.

[10] Woolley, D. W., H. A. Waisman, and C. A. Elvehjem, Nature and partial synthesis of the chick anti-dermatitis factor, *Jour. Amer. Chem. Soc.* **61**: 977, 1939.

[11] Axelrod, A. E., R. J. Madden, and C. A. Elvehjem, The effect of nicotinic acid deficiency upon the coenzyme I content of animal tissues, *Jour. Biol. Chem.* **131**: 85, 1939.

[12] Teply, L. J., F. M. Strong, and C. A. Elvehjem, The distribution of nicotinic acid in foods. *Jour. Nutr.* **23**: 417, 1942.

[13] Sebrell, W. H., Table showing the pellagra-preventive value of various foods, *Pub. Health Repts.* **49**: 754, 1934.

grits. The rats on such a diet failed to grow normally but did not exhibit any of the symptoms typical of pellagra. However, these animals responded to a supplement of nicotinic acid [14] and these results gave us the first demonstration of the need of nicotinic acid by rats (table 2). Previously no one had been able to produce a nicotinic acid deficiency in this species. When similar rations were used for the chick it was also possible to produce a nicotinic acid deficiency and in this species we [15] saw the development of tongue lesions very similar to those observed in dogs. While these observations were most interesting they did not explain why milk and eggs were highly effective in the treatment of pellagra. This explanation came when we replaced corn with other cereals. When this was done the animals grew normally in spite of the fact that some of the cereals supplied less nicotinic acid than did corn (table 3). Upon further investigation it was found that the quality of protein in these cereals was different from that of corn.

TABLE 2

EFFECT OF CORN GRITS ON NIACIN REQUIREMENT OF THE RAT

	Average gain per week gm.
Sucrose basal	25
Sucrose basal + 40% corn grits	4
Sucrose basal + 40% corn grits + 1 mg.% niacin	27
Sucrose basal + 40% corn grits + folic acid concentrates	5

TABLE 3

EFFECT OF VARIOUS CEREALS ON RAT GROWTH

Ration	Tryptophan content mg.%	Nicotinic acid content mg.%	Grams gained per week
Basal	180	0.01	29
Basal + 40% corn grits	118	0.27	7
Basal + 40% yellow corn	129	0.92	13
Basal + 40% polished rice	138	0.56	31
Basal + 40% whole rye flour	158	0.41	26
Basal + 40% rolled oats	168	0.41	28
Basal + 40% white flour	151	0.40	29

Zein, the protein in corn, was shown to be low in the amino acid tryptophan by Wilcock and Hopkins as early as 1906. When pure tryptophan was added to the diet a growth response equal to that secured with niacin resulted.[16] Other amino acids proved to be completely inactive. This observation was important from two other points of view: (1) a definite relationship between a vitamin and an amino acid had been established, and (2) it explained the early observations of Goldberger because the proteins of milk and eggs carry appreciable amounts of tryptophan. Actually, Goldberger and Tanner [17] tried cystine and tryptophan in 1922 and made the following conclusion: "In some preliminary therapeutic trials with amino acids the dermal lesions in each of two cases seemed to show a markedly favorable reaction to cystine; and in a third case a steady gain in weight, with some improvement in diarrhea, accompanied the administration of both cystine and tryptophane."

TABLE 4

GROWTH DEPRESSING EFFECT OF AMINO ACIDS

	Average weekly gain in grams	
	−Niacin	+Niacin
Basal (sucrose)	11	16
Basal + 2% acid hydrolyzed casein	4	16
Basal + 2% casein	17	20
Basal + 2.4% amino acid mixture	4	16
Basal + .156% dl-threonine	2	20
Basal + .104% dl-phenylalanine	5	17
Basal + 2% glycine	5	16
Basal + 2% dl-alanine	13	14

These results also discredited the old idea that corn contains a toxic factor because all the results can be explained on the nicotinic acid and tryptophan content of the ration, although Woolley claims that corn contains some factor which increases niacin or tryptophan requirements. It is unfortunate for the corn-eating people of the world that corn is low in both nicotinic acid and tryptophan. A similar condition can be produced by diets containing no corn but carrying an imbalanced ratio of amino acids [18] (table 4).

[14] Krehl, W. A., L. J. Teply, and C. A. Elvehjem, Corn as an etiological factor in the production of a nicotinic acid deficiency in the rat, *Science* **101**: 283, 1945.

[15] Sarma, P. S., and C. A. Elvehjem, Growth inhibition of chicks on rations containing corn grits, *Poultry Sci.* **25**: 39, 1946.

[16] Krehl, W. A., L. J. Teply, P. S. Sarma, and C. A. Elvehjem, Growth retarding effect of corn in nicotinic acid low rations and its counteraction by tryptophane, *Science* **101**: 489, 1945. Krehl, W. A., P. S. Sarma, and C. A. Elvehjem, The effect of protein on the nicotinic acid and tryptophane requirement of the growing rat, *Jour. Biol. Chem.* **162**: 403, 1946.

[17] Goldberger, J., and W. F. Tanner, Amino acid deficiency probably the primary etiological factor in pellagra, *Pub. Health Repts.* **37**: 462, 1922.

[18] Krehl, W. A., L. M. Henderson, J. de la Huerga, and C. A. Elvehjem, Relation of amino acid imbalance to niacin-tryptophane deficiency in growing rats, *Jour. Biol. Chem.* **166**: 531, 1946.

Again the animals respond to either the vitamin or the amino acid. The results can best be explained on the basis that tryptophan may function as a metabolic precursor of nicotinic acid and that certain amino acids interfere with this conversion. Rosen, Huff, and Perlzweig [19] found that the addition of tryptophan to the diet of rats on a low protein diet was followed by an immediate increase in the nicotinic acid excretion in the urine. Similar results have been obtained in humans [20] and tryphophan has produced some response in human pellagra. Therefore, this interrelationship appears to hold in all species of animals. The biosynthesis of nicotinic acid from tryptophan has also been studied in the mold neurospora.[21] All the intermediate reactions are not clearly understood but there can be no question regarding the transformation of tryptophan into nicotinic acid. In the animal body this transformation may take place both in the tissues of the animal and by the bacteria in the intestinal tract. The intestinal flora must play some part since the type of carbohydrate in the diet influences the results. The amino acid imbalance does not appear when dextrin is used in the diet.

In conclusion, I believe it is perfectly clear that pellagra is a deficiency disease. Goldberger came to this conclusion largely through experiments with human-beings. This courageous and ingenious scientist was assigned a difficult task, the solution of which lay in a field in which he had little training. He was forced to work under adverse conditions and was subjected to the most severe criticism. Nevertheless, by the application of epidemiological methods he was able to demonstrate that this disease was not only related to the diet but was due to shortages of unknown nutrients which were present in certain foods and lacking in others. All of his findings and most of his explanations have been verified with time. However, it is evident from this short history of pellagra that progress may be very slow without laboratory and animal experimentation. We have here another excellent example of significant contributions to human welfare through the proper use of the experimental animal.

[19] Rosen, F., J. W. Huff, and W. A. Perlzweig, The effect of tryptophane on the synthesis of nicotinic acid in the rat, *Jour. Biol. Chem.* **163**: 343, 1946.

[20] Sarett, H. P., and G. A. Goldsmith, The effect of tryptophane on the excretion of nicotinic acid derivatives in humans, *Jour. Biol. Chem.* **167**: 293, 1947; Perlzweig, W. A., F. Rosen, N. Levitas, and J. Robinson, Excretion of nicotinic acid derivatives after ingestion of tryptophane by man, *Jour. Biol. Chem.* **167**: 511, 1947; Sarett, H. P., and G. A. Goldsmith, Tryptophane and nicotinic acid studies in man, *Jour. Biol. Chem.* **177**: 461, 1949.

[21] Beadle, G. W., H. K. Mitchell, and J. F. Nye, Kynurenine as an intermediate in the formation of nicotinic acid by Neurospora, *Proc. Natl. Acad. Sci.* **33**: 155, 1947.

THE ADAPTATIONS OF MOSQUITOES TO THE TROPICAL RAIN FOREST ENVIRONMENT

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A LABORATORY was established at Villavicencio, in eastern Colombia, in 1938 under the joint auspices of the Ministry of Health of the Colombian Government and the Rockefeller Foundation, for the study of the epidemiology of sylvatic yellow fever. It had been discovered some years earlier that the disease was frequent in the region, although the usual mosquito vector, *Aëdes aegypti*, was not found there. The disease was closely associated with the forest, so that it could almost be called an occupational disease of woodcutters, and the situation seemed to be unusually favorable for study.

In the course of the Villavicencio work we arrived at a theory of the mechanism of maintenance of the disease that has been described at length in various papers (e.g., Bates, 1946). Essentially, it seems that the virus is maintained by constant passage among forest monkeys, with forest mosquitoes acting as vectors, the human infections resulting from man accidentally becoming involved in these forest cycles. Studies carried out simultaneously in Brazil gave similar results (Taylor and da Cunha, 1946), and the situation in East Africa has also been found to be curiously parallel, of course involving different mosquitoes and monkeys, but species with similar habits (Smithburn and Haddow, 1946).

In all of these places, the monkeys, the mosquitoes, and consequently the yellow fever virus, are inhabitants of the upper strata of the forest, of the forest canopy. The realization of this led us to undertake studies of the habits of the forest mosquitoes, and of the general characteristics of the forest environment. The object of the present paper is to describe, in a general way, some of these mosquito studies.

FOREST STRATIFICATION

Field studies indicated a particular forest mosquito, *Haemagogus spegazzinii* (called *H. capricornii* in the earlier papers) to be the probable vector of sylvatic yellow fever. Virus was recovered from wild caught specimens of this species in Brazil in 1938 by Shannon, Whitman, and Franca, and later by Bugher and his coworkers (1944) in Colombia. Attention was consequently centered on the behavior of this species, and when Doctor Jorge Boshell discovered that these Haemagogus mosquitoes were most abundant high in the trees, studies were undertaken of the general phenomenon of the stratification of organisms in the forest environment.

We built a series of ladders in trees in several types of forest, with platforms at various elevations where observations on environmental conditions could be carried out. One of these platforms is shown in figure 1. By making parallel captures of mosquitoes coming to bite at various heights, we found that each species of forest mosquito showed a characteristic altitudinal distribution, some spe-

FIG. 1. One of the platforms built for observations in the canopy zone of the forest at Villavicencio.

cies being found only in the forest floor zone, others only high in the canopy, with a few species showing a random distribution with no clear zone of preference.

We found also that there were vertical gradients in various environmental factors, such as light, temperature, and relative humidity. At mid-day light, of course, was least at the forest floor, increasing toward the canopy, but in an irregular fashion, owing to local peculiarities of foliage distribution. Temperature at mid-day was lowest at the forest floor, increasing with height. This gradient was reversed at night. Relative humidity was highest at mid-day at the forest floor, and decreased with elevation. At night humidity was always at saturation all through the forest.

Detailed figures on mosquito captures and environmental measurements in the Villavicencio area were published in a paper by Bates (1944). Similar studies in African forest have been published by Haddow, Gillett, and Highton (1947). Interesting studies of the vertical distribution of bromeliads and mosquitoes in Trinidad forests have been published by Pittendrigh (1948).

The majority of the tropical forest mosquitoes are diurnal, and relatively few are crepuscular or nocturnal. The time of activity, again, is characteristic for each species, but the diurnal kinds fall into two broad classes, those with a single peak of activity at mid-day, and those with two peaks of activity, one in the morning and the other in the afternoon, with a letup in the mid-day period. The canopy mosquitoes in general (there are many exceptions) show the single mid-day peak of activity, while the floor-zone mosquitoes are apt to show the type of cycle with morning and afternoon peaks.

We gradually acquired the impression that the controlling factor both in determining vertical distribution and cycle of activity, was relative humidity, though we have no clear experimental evidence for this. It is interesting that most of these forest mosquitoes are brightly colored, and that the species that fly at mid-day and at high levels are apt to be metallic green or blue. I suspect that this is an adaptation to the comparatively low humidities and high temperatures that prevail in the upper canopy zone of the forest. Some of these tropical mosquitoes are very colorful indeed, metallic blue with patches of silver, gold, or copper scales. One group (*Megarhinus*) has tufts of bright red scales along the metallic green abdomen.

We found that these metallic diurnal mosquitoes, particularly *Haemagogus*, were short-lived if kept in an atmosphere saturated with water vapor in the laboratory. At a constant temperature of 25° C., these mosquitoes, when kept in small cages in the moist atmosphere of a subterranean room, showed an average length of life of 7.6 days; at the same constant temperature, but with the mosquitoes confined in shell vials in a cabinet with a relative humidity of 60 to 70 per cent, the average length of life was 18.3 days (data in Bates, 1947). Mosquito species belonging to the forest floor zone fauna, such as *Aëdes serratus* and *Psorophora ferox*, showed exactly reverse relative survivals under these conditions. Laboratory transmission experiments with yellow fever virus in Haemagogus mosquitoes were long handicapped by our failure to realize the unfavorable effect of high humidity on the survival of the mosquitoes. Such experiments were carried out easily once we had found the environmental conditions that allowed the mosquitoes to survive for the completion of the incubation period of the virus.

FOREST BREEDING PLACES

During the latter part of my stay in Villavicencio, I became particularly interested in the factors governing the selection of different larval habitats by different species of mosquitoes. Interest in this problem arose because of investigations of the breeding habits of the anopheline mosquitoes acting as vectors of malaria. The anophelines, important as malaria vectors around Villavicencio, all lived in open habitats—marshes, ponds, and streams—but we returned to the study of forest mosquitoes because the environmental conditions in some of the forest habitats seemed more easily analyzed. An ecological survey was started, in collaboration with Doctor Lloyd Rozeboom, of the Johns Hopkins University, and we hope eventually to publish a detailed account of this work.

Many of the species collected in the course of this survey proved to be undescribed, and thus to require considerable taxonomic study, which Doctor Rozeboom is carrying out. The fauna proved to be very rich indeed. Over 150 species have already been identified from the Villavicencio material, which may be compared with a total fauna of 121 mosquito species known from the United States and Canada. Most of these mosquitoes were inhabitants of the forest.

We could find additional species almost any day that we went out with this purpose in view. A

productive method of adding to the list was to sit down in some comfortable spot in the forest and try to think of a situation in the neighborhood that might contain water, but that hadn't been explored. If we found a new variety of water-containing situation—the hollow stem of a tree, a water-holding flower, anything of that sort—it usually proved to be inhabited by some new kind of mosquito. There seemed to be no type of situation in the forest in which water could be held over between rains that was not utilized by some species or other of mosquito. With all of our searchings, I am convinced that we continued to overlook one of the major forest breeding places, because there was a whole series of mosquitoes that could be collected commonly as adults high in the forest canopy, whose larvae we never found.

The luxuriance of the forest mosquito fauna, then, is related to the variety of larval habitats available in the forest. These depend in turn on the variety of trees that go to make up the basic structure of the forest, and on the variety of epiphytes, vines, and detritus that accumulate around this basic structure. The forest biotic community is a sort of multiplication table of niches and inhabitants thereof, the whole reaching a considerable complexity.

I have tried in a recent book (Bates, 1949) to work out a general classification of the breeding places of mosquito larvae. By this system, four major habitat types are recognized:

A. Permanent or semipermanent standing water.
B. Running water.
C. Transient ground pools.
D. Container habitats.

The first three types are not of much interest in the present connection, though all three include a few characteristic forest habitats. Transient ground pools sometimes produce large quantities of mosquitoes in lowland tropical forests, even though the number of different kinds inhabiting such situations is limited. Also several generic groups are specialized for life in habitats associated with forest streams, among dead leaves in side eddies, or in tangles of roots. The majority of the different kinds of forest mosquitoes, however, breed in container habitats.

The term "container habitats" was proposed by Shannon (1931) for all of the various special plant situations that hold free water: water in rot-holes in trees, at the leaf bases of growing plants, in flowers, in fallen leaves or fruits, and so forth. He distinguished between these "natural

FIG. 2. The base of a fallen palm leaf serves as a water container, an example of the general class of "ground container" breeding places of forest mosquitoes.

containers" and "artificial containers" such as old automobile tires and tin cans. Both tires and cans were absent from the Villavicencio forests, though we made some interesting observations on the inhabitants of containers of our own manufacture.

Rot-holes in trees are the most widespread type of container habitat. They occur in the temperate zone as well as in the tropics, and form the habitat of a specialized though limited mosquito fauna in both North America and Europe. Tree-hole faunas are particularly rich everywhere in the tropics. What species is found in a particular tree-hole seems to depend on the nature of the biotic community in which the habitat occurs, on the size of the opening, on its height above ground, and on the condition of the water, whether clear or with a high content of dissolved organic materials.

A large class of tropical forest larval habitats is covered by the term "ground container." Many things that fall to the forest floor may hold water. In the Villavicencio region the commonest containers were palm leaf bases (fig. 2), palm flower bracts, and a few types of fallen leaves. Fallen tree trunks and bamboo stems may also hold water. In other parts of the world snail shells and fruit husks may be important ground container habitats. The water in these containers is sometimes foul with decaying detritus, and the mosquito fauna in that case is very different from the fauna in containers with clear water.

The Neotropical plant family of Bromeliaceae form one of the most commonly studied of container habitats. The expanded leaf bases of these plants form water-tight "tanks" which in some cases may hold several liters of water (fig. 3). The water and accompanying organic material serve the bromeliad as a source of food, and incidentally maintain a rich and specialized aquatic biota. Many types of aquatic insects, as well as amphibia and other aquatic organisms, develop in these bromeliad tanks. The various bromeliad species, like the mosquitoes, show stratification in the forest environment, and the mosquito fauna of a particular plant apparently depends on the kind of plant, and on its location in the forest. The pertinent literature on these plants has been summarized by Pittendrigh (1948).

Many other plants, particularly of the families Araceae and Musaceae, have tropical forest species that accumulate water at the leaf bases in which mosquito larvae live. In the Villavicencio region, we found generally that each kind of aroid or musaceous plant had its characteristic species of mosquito. Haddow (1948) has published detailed studies of the mosquitoes breeding in plant axils in an African forest. He did not find the correlation between kind of mosquito and kind of plant that we observed in Villavicencio, but the African axil-inhabiting mosquito fauna seems also to be less extensive and less specialized than the corresponding South American fauna.

FIG. 3. The wide, overlapping leaf bases of bromeliads form a "tank" in which water and debris collects, forming a special aquatic habitat in tropical American forests.

Several forest plants have upright flower spikes with bracts that collect water, thus providing mosquito breeding places. The flowers of the common *Heliconia* of the Villavicencio region were inhabited by two closely similar species of the mosquito genus *Wyeomyia*. The idea that two hardly distinguishable species should have an identical larval habitat conflicts with my conceptions of population isolating mechanisms, but the taxonomists are sure that two species are involved. As is usual for a tropical situation, experimental evidence on the status of the populations is lacking. Larvae in these flower bracts are sometimes

very numerous: there may be hundreds of first stage larvae and twenty-five or thirty full grown larvae in a single flower spike, containing two or three cc. of water.

The fauna that interested me most was that of the bamboo internodes. It is as rich, in the Villavicencio area, as the bromeliad fauna, much less well known, yet in many ways easier to study. More than twenty mosquito species were found breeding in bamboo internodes and nowhere else: mostly very peculiar species, many undescribed. Figure 4 shows one of these bamboo breeding places. The original entrance to the internode shown here was the small worm hole below the pipette. The hole in which the pipette is inserted was made with an augur and kept closed with a cork, for the purpose of making regular larval collections from the same bamboo joint.

In any given bamboo internode there would usually be only two or three species of mosquitoes, and the factors that governed this distribution presented a nice problem for study. The relation seemed to be with the size of the opening into the internode, the height on the bamboo stem, the condition of the water (which was related to the age of the opening) and the type of forest in which the bamboo clump was located.

The mosquitoes in some of these cases entered through very tiny worm holes. In a few instances, we found larvae in internodes where we could find no entrance at all, which at least shows that the entrance must have been obscure. The adult mosquitoes show nicely corresponding behavior adaptations. Most adult mosquitoes are easily kept in test tubes stoppered with cotton plugs, but some of these bamboo mosquitoes would wriggle up into such a plug, usually in the end getting entangled and killed, but sometimes escaping. They could not be confined in cages closed with the usual wire screen. Normal mosquitoes bump against a screen and give up, but these would stick their heads into the mesh and wriggle through.

FIG. 4. The water that collects in bamboo internodes, when these have been punctured by boring insects or birds, forms the habitat of a specialized and distinctive mosquito fauna.

BEHAVIOR ADAPTATIONS

The relation between adult behavior and the characteristics of the larval breeding place in the case of the bamboo mosquitoes is but one example of the interrelationships of larval and adult adaptations shown by these forest mosquitoes. This is a rich field for study which would surely repay intensive cultivation.

There is, for instance, a clear relation between the rate of growth of the larvae and the type of habitat. Some of the species that live in very transitory water accumulations, such as ground pools of rain water or some of the ground containers, show incredibly rapid growth, reaching maturity in four or five days. On the other hand, the larvae that live in very constant and protected habitats, like the film of water under the leaf sheaths of the banana-like *Ravanela* plants, may take two or three months to reach maturity.

Then there is the problem of the maintenance of the species through periods of adversity, a problem that varies with the nature of the habitat. Rot-holes in trees exist for many years, with a fluctuating content of water depending on the seasonal rains. Tree-hole breeders very generally take advantage of this by maintaining themselves through dry periods by a diapause in the egg stage, the eggs being plastered on the walls of the tree-hole and not hatching until submerged. Eggs of such species may remain viable for a year or more, hatching only in response to some particular stimulus, the exact nature of which depends on the mosquito species involved.

This solution of the problem is useless for the ground container breeders, since fallen leaves and flower bracts do not last from one season to the next. I am sure that many of these ground container breeders (such as *Trichoprosopon* and *Carrollia*) depend on adult longevity for the maintenance of the species. The adults may be extraordinarily long-lived and tough, surviving in laboratory cages for months with little care or attention.

We became very interested in the relation between the "alarm reaction" of larvae and the type of larval habitat. Everyone has noticed that the common domestic mosquito wrigglers drop from

the surface of the water if a shadow strikes them. This seems very generally to be a response to a drop in light intensity, the larvae being indifferent to even a large and sudden increase in light intensity. The amount of drop in intensity necessary to initiate the reaction varies in different species, some being exceedingly sensitive, reacting to light changes imperceptible to the eye. The interesting point in the present connection is that larvae found in exposed situations, such as open ground containers, proved to be most sensitive to light change. Species breeding in stream habitats proved to be indifferent to light manipulation—if such larvae dodged at every shadow they would be in danger of being caught by the stream current and carried away. The species from bamboo internodes were also completely indifferent to our light manipulations— they live, of course, in perpetual darkness, completely protected from outside predators.

FOREST CHARACTER OF THE NEOTROPICAL BIOTA

Almost all groups of organisms in the Neotropical biota are branded with the mark of the forest. The gallinaceous birds, for instance, have taken to the trees. Mammals of very diverse orders have developed forest adaptations like prehensile tails. Plants of a variety of families have developed the liana habit. Members of an even wider range of plant families, from the grasses (bamboo) to the composites, have adopted giantism, or the tree form, which seems to me essentially an adaptation for getting to the light supply at the top of the forest. I haven't the statistics, but surely no other part of the world can show anything like the variety of epiphytes, and one great epiphytic family, the bromeliads, is exclusively Neotropical.

The mosquitoes also carry this brand. The Neotropical mosquito fauna is the richest in the world, with 501 species listed in the latest catalogue (Lane, 1939). This may be compared with 358 species known from the Ethiopian region, which has been much better studied. The Neotropical fauna is also the most distinct of the regional mosquito faunas. The world's mosquitoes have been divided into 88 generic stems (treated as subgenera by mosquito taxonomists). Of these, 23 are peculiar to the Neotropical region, with 6 others extending into the southern Nearctic. Australia, with the next most distinctive fauna, has only 11 peculiar generic types, with 3 more that have spread into the Oriental region.

This peculiar Neotropical fauna is closely associated with the forest. Only 3 of the 29 indigenous stems show any great proliferation of species in open country breeding places like ponds and marshes. The other groups, which include the majority of the species of the total fauna, breed exclusively in forest habitats, mostly in plant containers.

These forest types include some groups, such as *Trichoprosopon*, that on morphological grounds appear to be the most primitive of living mosquitoes. It has been suggested, on these grounds, that container habitats like tree-holes may represent the primitive type of mosquito breeding place. I rather doubt this. It seems to me likely that the large, morphologically primitive, Neotropical mosquito fauna has taken to forest habitats because these are the most available habitats in the geographical area, furnishing an ecological diversity that enables a great many diverse specific populations to survive side by side.

The Neotropical forest biota includes many endemic types of organisms that, on morphological grounds, can be considered "primitive"—examples could be found in almost all of the classes of terrestrial animals, from marsupials to *Peripatus*. There seems to have been a great proliferation of species within the forest environment, indicating a considerable degree of evolution at this level. There is little evidence, however, to contradict the thesis of Matthew (1939) that the origin of basic new organic types among terrestrial animals has largely taken place in the large Asiatic temperate zone land mass. The characteristics of evolution within the tropical rain forest environment, however, would certainly repay much more detailed investigation, from either the observational or experimental points of view, than has yet been given the subject.

CONCLUSIONS

The thesis of this rather sketchy account of mosquito work in the rain forest of the Villavicencio area is that more intensive study, both of mosquitoes and of the forest, would surely be productive in the development of general biological theory. Almost all of our information about the tropical forest is based on observation, unchecked by experiment, in large part carried out by travelers or temporary residents. The urgent need for information concerning the mechanism of maintenance of sylvatic yellow fever enabled the staff of the Villavicencio laboratory to carry out continuous forest work over a ten year period. This sustained program enabled us to obtain useful information about the immediate and specific

problem, but it also served to tantalize us with a vista of the endless further problems that await scientific attention.

The coral reef and the rain forest are, I think, the two most fascinating environments on our globe for the naturalist. At least they are the richest environments, in numbers of kinds of organisms if not in numbers of individuals, and they thus may be presumed to represent optimum conditions for life. Yet they have been little studied. They are tropical environments, and the great universities and research laboratories, which contain the scholars who might analyze the phenomena of the forests and the reefs, are located in the temperate zone. This is, I think, a very unfortunate accident of allopatric geographical distribution which I hope somehow, in the long run, can be corrected.

REFERENCES

BATES, M. 1944. Observations on the distribution of diurnal mosquitoes in a tropical forest. *Ecology* **25**: 159–170.

—— 1946. The natural history of yellow fever in Colombia. *Scientific Monthly* **63**: 42–52.

—— 1947. The development and longevity of haemagogus mosquitoes under laboratory conditions. *Ann. Ent. Soc. Amer.* **40**: 1–12.

—— 1949. The natural history of mosquitoes. N. Y., Macmillan.

BUGHER, J. C., J. BOSHELL-MANRIGUE, M. ROCA-GARCIA, AND E. OSORNO-MESA. 1944. Epidemiology of jungle fever in eastern Colombia. *Amer. Jour. Hyg.* **39**: 16–51.

HADDOW, A. J. 1948. The mosquitoes of Bwamba County, Uganda. VI. Mosquito breeding in plant axils. *Bull. Ent. Res.* **39**: 185–212.

HADDOW, A. J., J. D. GILLETT, AND R. B. HIGHTON. 1947. The mosquitoes of Bwamba County, Uganda. V. The vertical distribution and biting cycle of mosquitoes in rain forest with further observations on microclimate. *Bull. Ent. Res.* **37**: 301–330.

LANE, J. 1939. Catálogo dos mosquitos neotrópicos. *Bol. Biol. Ser. Monogr.*, No. 1, São Paulo.

MATTHEW, W. D. 1939. Climate and evolution. 2nd. ed. N. Y., Acad. Sci., special publication.

PITTENDRIGH, C. S. 1948. The bromeliad-anopheles-malaria complex in Trinidad. I. The bromeliad flora. *Evolution* **2**: 58–89.

SHANNON, R. C. 1931. The environment and behavior of some Brazilian mosquitoes. *Proc. Ent. Soc. Washington* **33**: 1–27.

SHANNON, R. C., L. WHITMAN, AND M. FRANCA. 1938. Yellow fever virus in jungle mosquitoes. *Science* **88**: 110–111.

SMITHBURN, K. C. AND A. J. HADDOW. 1946. Isolation of yellow fever virus from African mosquitoes. *Amer. Jour. Trop. Med.* **26**: 261–271.

TAYLOR, R. M. AND J. FONSECA DA CUNHA. 1946. An epidemiological study of jungle yellow fever in an endemic area in Brazil. *Amer. Jour. Trop. Med.* **26**; supplement.

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